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TRANSACTIONS

OF THE

INDIANA

STATE MEDICAL SOCIETY,

AT ITS

SIXTEENTH ANNUAL SESSION,

HELD AT

INDIANAPOLIS, MAY 15, 16 AND 17, 1866.

PUBLISHED FOR THE SOCIETY.

INDIANAPOLIS:
DOUGLASS & CONNER, PRINTERS.
1866.

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TRANSACTIONS

OF THE

INDIANA STATE MEDICAL SOCIETY,

AT ITS

SIXTEENTH ANNUAL SESSION.

INDIANAPOLIS, IND., May 15, 1866,
2 O'CLOCK P. M.

The Indiana State Medical Society assembled in College Hall, and was called to order by the President, M. H. Harding, M. D.

The President appointed Wm. Lomax, R. N. Todd, and F. S. Newcomer a Committee on Admission.

The Executive Committee made the following report, which was adopted:

The Executive Committee report the following Order of Business for the Society; and would state that they have procured College Hall for the meetings of this Society; and have made arrangements with the railroads, as far as possible, for returning members, who have paid full fare in coming to the meeting of the Society, to their homes free of charge.

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ORDER OF BUSINESS.

1. Reports of Officers.
2. Appointment of Committees.
3. Reports of Committees.
4. President's Address.
5. Miscellaneous Business.
6. Volunteer Papers.

J. H. WOODBURN, *Chairman.*
T. B. HARVEY,
J. M. GASTON.

The following Treasurer's Report was adopted:

TREASURER'S REPORT.

To balance from last year....		$12 49
To cash of W. F. Harvey, Secretary..... ...		139 00
To two dollars each from Joseph A. Nesbit, Richard Brand, R. G. Graydon, N. H. Canaday, A. L. Underwood, G. W. Mears, W. Bracken, M. H. Field, M. H. Wright, Luther Jewett, John Rea, J. M. Ireland, Isaac M. Rosenthal, F. S. Newcomer, N. P. Howard, W. F. Green, D. W. Crouse, C. M. Wetherell, Wm. Reader, John Sloan, Wm. Gramm, W. N. Duzan, W. A. Clapp, J. W. Moody, J. Isler, E. H. Crippen, T. R. Austin, S. H. Charlton, S. Linton, Wm. Scott, A. Leslie, G. M. Darrach, Wesley Allen, R. Winton, W. R. Winton, A. D. Gall, J. McWorkman, J. W. Hervey, J. S. Bobbs, A. Chapman, J. M. Gaston, W. C. Thompson, W. H. Wishard, S. D. Day, M. Sexton, H. H. Moore, Jos. Stephenson, J. A. Comingore, L. Ritter, James Thampson, H. Winton, W. B. Fletcher, G. W. Slack, A. G. Boynton, R. B. Jessup, G. V. Woolen, J. M. Jessee, A. J. McLeod, D. Wiley, D. H. Oliver, H. C. Cole, T. M. Stevens, Wm. Spencer, G. W. Duzan, A. G. Preston, P. McNab, A. W. Reagan, R. M. Welman		136 00
		$287 49
Credit by cash paid Executive Committee.....	$15 80	
" " " Hall Rent...	8 00	
" " " Elder, Harkness & Bingham, printing,	121 90	
" " " Executive Committee.....	10 50	
" " " M. Burton, Hall Rent and Janitor..	11 00	
" " not received of A. Chapman.....	2 00	
		$183 20
Balance on hand.		$104 29

J. H. WOODBURN, *Treasurer.*

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The President appointed as a Committee on Finance, H. B. Ayres, G. W. Clippinger, and J. A. Comingore.

The Committee on Publication reported as follows:

The Committee on Publication report that the Transactions were delayed on account of the absence of the chairman, and the separation of the committee. The cost of Transactions, Circulars, &c., which was paid by the Treasurer, was as follows:

300 copies Transactions	$121 90
300 Circulars	5 00
200 three cent and 50 two cent Postage Stamps	7 00
200 Envelopes	2 00
Total	$135 90

The circulars were printed and sent to delinquents at the request of the Secretary.

T. Parvin's paper on Pelvic Celulitis was withheld from publication at his own request.

The errors in President Lockhart's address resulted from a misunderstanding, in consequence of which the proof was never corrected.

To avoid similar errors in future, it is suggested that the author correct his own proof, or manuscripts be made more legible.

W. P. PARR, *Chairman.*

J. S. Athon presented the Credentials of A. Dunlap, M. D., and W. P. Kincaid, M. D., as delegates from the State Medical Society of Ohio, and, on motion, they were invited by the President to participate in the deliberations of the Society.

T. Parvin suggested that Dr. Dunlap be invited to address the Society on Ovariotomy, and Dr. Kincaid on any subject he chose. The latter addressed the Society for a short time on The Medical Profession.

The Committee on Surgery of the War was called for, but was not ready to report this year.

V. Kersey presented a paper, as Committee on Cholagogues, which was, by request, read by D. Clark, and discussed by J. F. Hibberd and V. Kersey, and referred to the Committee on Publication.

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The Committee on Finance reported as follows, which was adopted.

The Committee on Finance having examined the report of the Treasurer, J. H. Woodburn, find his exhibit correct, and recommend its acceptance; and further recommend that a fee of two dollars be paid by the members of the Society.

H. P. AYRES, *Chairman.*

The President appointed the following Committee on Nominations: R. N. Todd, R. E. Haughton, and R. A. Curran.

The Society adjourned to meet at 8 o'clock P. M.

EVENING SESSION.

The Society met at 8 o'clock P. M.

The President delivered his inaugural address.

On motion of W. Lomax, the thanks of the Society were tendered to the President, and the address referred to the Committee on Publication.

W. Lomax offered the following resolution, which was adopted:

Resolved, That it is the sense of this meeting that the State Medical Society, should be changed from a primary voluntary association to a representative body, to be composed of delegates from County Societies, and that a committee be appointed to report a plan for effecting such change.

The President appointed W. Lomax, R. A. Curran, W. R. Winton, H. P. Ayres, and T. B. Harvey as the committee.

The President appointed V. Kersey, J. H. Woodburn, and W. Lomax a Committee on Ethics.

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The committee to report a plan for changing the character of the Society to a delegate body, made a report, which was made the special order for 8 o'clock to-morrow morning.

The Committee on Ethics made the following report, which was accepted.

The Committee on Ethics, to whom was referred the application for membership of O. Aborn, agree to report that we have examined the facts, and heard the statement of the applicant, and believe it would be improper to admit him to membership at this time, but would favor his admission next year, in case his professional record should be satisfactory in the future.

V. KERSEY,
J. H. WOODBURN, } *Com. on Ethics.*
W. LOMAX.

The subject of Cholera was made the order for 9 o'clock A. M., to-morrow.

All members having volunteer papers were requested to report them to the Secretary.

The following Committee on Prize Essays was appointed: J. F. Hibberd, H. P. Ayres, and W. B. Fletcher.

A motion to so change the By-Law on the subject of Prize Essays, that where the word "money" occurs, the word "value" shall be substituted, was adopted.

The report on changing the organization of the Society, was called up, and discussed by H. Winton, J. H. Woodburn, R. A Curran, M. H. Harding, L. R. Johnson, and W. Lomax.

Adjourned to meet at the Hall of Representatives at 8 o'clock A. M., May 16.

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SECOND DAY.

MAY 16, 1866.

The Society met in Representative Hall, at 8 o'clock A. M. The President in the Chair.

The report on change of organization was referred to the committee, with instructions to so change it as to allow all old members to retain their present rights and privileges.

The committee were further instructed to add the word "local" to "county," in Art. iii.

The committee then presented their report with the changes ordered.

On motion, L. D. Waterman read the report, which was adopted, seriatim, by the Society, with some amendments, after discussion. The report was then adopted as a whole.

The rules were suspended, and A. Dunlap, of Springfield, Ohio, addressed the Society on Ovariotomy.

The thanks of the Society were given to Dr. Dunlap for his address.

The subject of Dr. Dunlap's remarks was discussed by T. B. Harvey and R. E. Haughton, and questions by various gentlemen answered by Dr. Dunlap.

The President appointed L. R. Johnson, S. D. Day, and F. S. Newcomer a Committee on Obituaries.

L. R. Johnson read a paper on Cholera.

R. E. Haughton then commenced the reading of a paper on Cholera, but a motion to suspend the further reading until after dinner and adjourn, carried.

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AFTERNOON SESSION.

The Society met at 2 o'clock P. M. The President in the Chair.

The Committee on Obituaries offered the following report:

The Committee on Obituaries report, that since the last session of this Society, H. G. Sexton, M. D., of Rushville, an honored member of the Society, has passed from the scene of his earthly labors to a higher sphere of existence and duty. Dr. Sexton was a pioneer of our profession in this State; was for nearly half a century identified with its interests, and shared its labors and honors; and at the close of his life was engaged in active practice. In his death, not only the profession, but the community in which he resided, has sustained a heavy loss. We recommend the adoption of the following resolutions:

Resolved, That this Society has heard, with deep regret, of the death of H. G. Sexton, M. D., an active, useful, and honored member of the profession and of this Society; to whose family, as to the community in which he resided, we tender our profound sympathy.

Resolved, That these proceedings be entered upon the records of this Society, and a copy thereof transmitted, by the Secretary, to the family of the deceased.

L. R. JOHNSON, *Chairman.*

The resolutions were adopted.

The following names of deceased members were also reported, and resolutions, similar to the above, adopted:

B. C. Rowan, M. D., of Fort Wayne.
Thos. A. Wilson, M. D., of Brownstown, (died in 1863).
J. J. Kimberlin, M. D., of Medora, (died in 1863).
Robert Charlton, M. D., of Indianapolis, Ind.

In reference to the last named gentleman, the following history was given by his brother, S. H. Charlton:

DIED.—On the 21st of April, 1865, of phthisis pulmonalis, Robert Charlton, M. D., aged 32 years.

Dr. Charlton was Surgeon of the 79th Ind. Reg't until his health failed, and being unable to resume active civil practice, was connected with the military hospitals at Nashville and Madison, and afterwards as Assistant Physician at the Indiana Hospital for the Insane. He was a worthy member of the profession, and a consistent christian.

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A motion was carried to adjourn *sine die* at 12 o'clock tomorrow.

R. E. Haughton then finished the reading of his paper on Cholera.

Wm. F. Harvey read a paper on Cholera.

All the papers on Cholera were referred to the Committee on Publication, and speakers on the subject limited to ten minutes.

The subject was discussed by W. P. Kincaid, T. C. Eakin, A. G. Preston, L. R. Johnson, R. E. Haughton, V. Kersey, H. P. Ayres, J. F. Hibberd, Wm. Lomax, J. W. Hervey, and Wm. F. Harvey.

The following was adopted:

Resolved, That the By-Law, requiring the names of delinquents to be published with the Transactions, be so changed as to read, "The names of all the members who fill the requirements of the Constitution and By-Laws of this Society, and all honorary members, shall annually be published with the transactions.

The following By-Law was adopted:

Section 4. The Secretary is hereby authorized to employ a shorthand reporter, at an expense not exceeding ten dollars a day, to report and transcribe all the proceedings of the Society; and said report, after having been condensed by the Committee on Publication, shall be published in the Transactions. The Secretary shall prepare an abstract of the expense incurred in preparing said report, and said expense shall be assessed, *per capita*, upon the members, and collected in the same manner as the annual dues.

A motion, to appoint a committee to present to the Legislature the petition recommended by Dr. —. Hawkins, after free discussion, was laid on the table.

James Thompson read a report of surgical cases, which was referred to the Committee on Publication.

The Committee on Nominations made the following report for officers of the Society for the ensuing year:

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PRESIDENT.

VIERLING KERSEY, M. D., Richmond.

VICE PRESIDENT.

J. H. WOODBURN, M. D., Indianapolis.

SECRETARY.

L. D. WATERMAN, M. D., Indianapolis.

ASSISTANT SECRETARY.

HORACE WINTON, M. D., North Manchester.

CORRESPONDING SECRETARY.

J. K. BIGELOW, M. D., Indianapolis.

TREASURER.

WILLIAM B. LYONS, M. D., Huntington.

The report was accepted, and the gentlemen elected.
On motion, adjourned to 7½ o'clock P. M.

EVENING SESSION.

The Society met at 7½ o'clock P. M. The President in the Chair.

The Treasurer presented claims against the Society, for the payment of which orders on the treasury were ordered to be drawn. (See Treasurer's Report).

H. C. Cole read a paper on Medical Progress, which was referred to the Committee on Publication.

T. Parvin read a paper on Vesico-vaginal Fistules, which was referred to the Committee on Publication.

For the purpose of partaking of an entertainment at the Bates House, given by the Indianapolis Academy of Medicine, the Society adjourned until 9 o'clock to-morrow morning.

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THIRD DAY.

MAY 17, 1866.

The Society met at 9 o'clock A. M. The Vice-President, T. B. Harvey, in the Chair.

The officers of the Society were instructed to furnish certificates to all members who desire to attend the State Medical Society of Ohio. The following were elected delegates: J. H. Townsend, W. B. Lyon, H. D. Wood, P. McNabb, L. Ritter, and A. W. Reagan.

The nomination of delegates to the American Medical Association was referred to a committee of three, as follows: H. B. Ayres, V. Kersey, and R. A. Curran.

N. Field offered the following resolution, which was adopted:

Resolved, That this Society recommend to the Medical Profession throughout the State, to organize County Medical Societies, auxiliary to the State Medical Society; and that they annually send delegates to represent them in the State organization; and that, as soon as organized, they report to the Secretary of the State Medical Society.

John A. Comingore read a paper on Excision of Bone, which was referred to the Committee on Publication.

Remarks were made on the paper by V. Kersey, T. C. Eakin, T. M. Stevens, M. Sexton, B. Merritt, and W. Lomax.

The following resolution was adopted:

Resolved, That all the Army Surgeons of the State are hereby requested by the State Medical Society to collect and transmit, timely, to Dr. Wm. Lomax, of Marion, Grant county, all facts of interest as to the Surgery of the War, to be compiled by him in the form of a report upon that subject, for the next meeting of this society.

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The following delegates to the American Medical Association were appointed:

Nathan Johnson,
J. E. Lyons,
F. S. C. Grayston,
Wm. Spencer,
A. G. Preston,
W. R. Winton,
R. N. Todd,
John Sloan,
D. D. Hall,
Wm. Lomax,
S. H. Charlton,
H. Hunt.
H. C. Cole,
P. McNab,
Geo. Harding,
J. S. Bobbs,
H. F. Barnes,
T. M. Stevens,
C. Richmond,
J. S. Athon,
R. E. Haughton,
H. D. Wood,
W. Thacker,
J. H. Townsend,
B. Merritt,
Joel Pennington,
Elias Fisher.

W. B. Fletcher being absent on account of sickness, his paper on Human Entozoa was referred, without reading, to the Committee on Publication.

The following committees were appointed:

COMMITTEE ON ARRANGEMENTS.

F. S. Newcomer, R. N. Todd, John A. Comingore, P. McNab, and James S. Athon.

COMMITTEE ON CREDENTIALS.

W. R. Winton, J. M. Kitchen, W. B. Fletcher, Lemuel R. Johnson, and W. Lockhart.

COMMITTEE ON FINANCE.

J. H. Woodburn, H. P. Ayres, D. H. Oliver, R. E. Haughton, and A. W. Reagan.

COMMITTEE ON ETHICS.

F. S. C. Grayston, Joel Pennington, James F. Hibberd, John S. Bobbs, and R. A. Curran.

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COMMITTEE ON PUBLICATION.

L. D. Waterman, W. B. Lyons, Wm. F. Harvey, T. M. Stevens, and G. V. Woolen.

The Vice-President in the Chair, appointed the following committees:

Cholagogues—Jas. F. Hibberd.
Cholera—G. Sutton.
Pathology of the Collapsed Stage of Cholera—W. S. Haymond.
Surgery—W. Lomax.
Obstetrics—G. W. Mears.
Inflammation—J. S. Bobbs.
Duality of Syphilitic Virus—Wm. Spencer.
Dysentery—H. P. Ayres.
Cerebro-spinal Meningitis—John Moffatt.

The Committee on Prize Essays was directed to furnish the Secretary with the subject, and the amount of the prize.

On motion, the section in the former By-Laws, in regard to prize money, was adopted as a By-Law under the present Constitution.

W. P. Kincaid and A. Dunlap were elected Honorary members of the Society.

The following members paid their annual assessment to the Secretary:

NAME.	POST OFFICE.
H. C. Cole	Kokomo.
John Lewis	Ogden.
D. D. Hall	Connersville.
A. C. Chapman	Alquina.
Isaac Mendenhall	Newcastle.
John Rea	Newcastle.
Nathan Mendenhall	Thorntown.

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NAME.	POST OFFICE.
J. W. Medaris	Brookston.
J. H. Townsend	Harland.
Wesley Allen	West Newton.
Joseph Stephenson	Pendleton.
C. N. Blount	Tipton.
J. R. Hinkle	Sullivan.
M. H. Harding	Lawrenceburg.
John Arnold	Rushville.
L. J. Francisco	Richmond.
William Spencer	Monticello.
William F. Riley	Sardinia.
R. H. Buck	Kokomo.
W. A. Pugh	Rushville.
J. R. Featherston	Bridgeport.
Horace Winton	North Manchester.
A. G. Boynton	Elizabethtown.
W. R. Winton	Wabash.
S. W. Purviance	Crawfordsville.
J. A. Comingore	Indianapolis.
Wm. Scott	Kokomo.
James Cochran	Spiceland.
J. M. Kitchen	Indianapolis.
J. M. Bell	Dublin.
Lewis Williams	Marion.
G. W. Slack	Yorktown.
G. V. Woolen	Indianapolis.
T. C. Eakin	Monroeville.
George Sutton	Aurora.
William Lomax	Marion.
H. P. Ayres	Fort Wayne.
James Thompson	Moscow.
E. H. Crippen	Moscow.
Vierling Kersey	Richmond.
Joel Pennington	Milton.
A. L. Underwood	St. Paul.

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NAME.	POST OFFICE.
R. B. Jessup	Vincennes.
F. S. Newcomer	Indianapolis.
Dougan Clark	Richmond.
J. H. Woodburn	Indianapolis.
G. W. Duzan	Zionsville.
M. Sexton	Rushville.
Wm. N. Duzan	Indianapolis.
S. D. Day	Shelbyville.
Wm. F. Harvey	Plainfield.
T. B. Harvey	Indianapolis.
J. S. Athon	Indianapolis.
D. H. Oliver	Indianapolis.
Jas. F. Hibberd	Richmond.
M. H. Wright	Indianapolis.
M. H. Field	Tampico.
R. G. Graydon	Southport.
R. Brand	Burnsville.
S. H. Charlton	Seymour.
J. W. Martin	Lebanon.
H. F. Barnes	Indianapolis.
W. B. Fletcher	Indianapolis.
J. I. Rooker	Castleton.
A. J. McLeod	Lebanon.
A. G. Preston	Greencastle.
P. McNab	Indianapolis.
R. N. Todd	Indianapolis.
A. W. Reagan	Mooresville.
J. N. Parr	Jolliette.
G. W. Clippinger	Indianapolis.

The following named members were not present, but sent in their assessment:

J. H. Brower	Lawrenceburg.
S. B. Harriman	Richmond.

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The Committee on Admissions reported the following physicians as having given satisfactory evidence of the required qualifications for membership in the Society, and they were elected members:

NAME.	POST OFFICE.
H. E. Dykeman	Fort Wayne.
Wm. H. Thacker	Fort Wayne.
A. H. Hunt	Fort Wayne.
Wm. H. Brooks	Fort Wayne.
T. C. Eakin	Monroeville.
G. T. Brueback	Fort Wayne.
N. L. Wickersham	Anderson.
S. S. Horne	Jonesboro.
J. A. Meek	Jonesboro.
Jas. S. Shively	Marion.
C. Lomax	Marion.
L. Corey	Warren.
H. Charles	Jonesborough.
L. T. Hess	Marion.
H. D. Reasoner	New Cumberland.
J. C. Helm	Muncie.
R. Winton	Muncie.
F. J. Bowles	Muncie.
G. W. Zimmerman	Wheeling.
Samuel V. Jump	
J. A. Stillwell	Brownstown.
C. V. N. Lent	Liberty Mills.
S. W. Thompson	Evansville.
R. R. Washburn	Waldron.
Wm. B. Lyons	Huntington.
J. E. Lyons	Huntington.
D. S. Lyman	Huntington.
R. A. Curran	Huntington.
J. M. Phipps	Indianapolis.
R. M. Wellman	Jasper.

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NAME.	POST OFFICE.
J. A. Fitzgerald	Indianapolis.
J. H. McIntyre	Richmond.
L. D. Waterman	Indianapolis.
John S. Forbes	Shelbyville.
B. Merritt	Indianapolis.
W. J. Elstun	Indianapolis.
T. J. Adams	North Salem.
Lemuel R. Johnson	Cambridge City.
J. K. Bigelow	Indianapolis.
C. Mayer	Fort Wayne.
H. D. Wood	Metz.
L. D. Robinson	Coatsville.
F. M. Hiett	Williamsport.
W. S. Haymond	Monticello.
C. Richmond	Kokomo.
J. W. Powell	Camden.
William Wands	Indianapolis.
J. M. Darnall	Kokomo.
L. Kern	Alto.
W. Q. Insley	Terre Haute.
J. J. Thompson	Sullivan.
H. C. Vincent	Guilford.
M. V. B. Newcomer	Tipton.
E. M. Morrison	Noblesville.
J. T. Floyd	Noblesville.
Nathaniel Field	Jeffersonville.
Jas. W. F. Gerrish	Seymour.
A. H. Bryan	Indianapolis.
W. K. Mavity	Kokomo.
C. S. Burr	Anderson.
David Wall	Clermont.
J. T. Strong	Plainfield.

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HONORARY MEMBERS.

NAME.	POST OFFICE.
R. J. Breckinridge	Louisville, Ky.
H. G. Carey	Indianapolis, Ind.
C. G. Comegys	Cincinnati, O.
P. M. Crume	Eaton, O.
Geo. Fries	Cincinnati, O.
J. McF. Gaston	South Carolina.
T. W. Gordon	Bethel, O.
R. D. Mussey	Cincinnati, O.
A. McAlister	Cincinnati, O
J. J. McIlhenny	Dayton, O.
W. C. Sneed	Kentucky.
E. B. Stevens	Cincinnati, O.
Rev. Dr. Talbott	Indianapolis, Ind.
Rt. Rev. Geo. Upfold	Indianapolis, Ind.
L. P. Yandell	Louisville, Ky.
M. Goldsmith	Louisville, Ky.
J. W. Benson	Louisville, Ky.
B. M. Wible	Louisville, Ky.
Geo. C. Blackman	Cincinnati, O.
T. Parvin	Indianapolis, Ind.
Edwin Hadley	Harveysburg, O.
A. H. Stephens,	Eaton, O.
Robert Woody	Eaton, O.
M. H. Haynes	Seven Mile, O.
J. R. Mendenhall	Richmond, Ind.
A. Dunlap	Springfield, O.
W. P. Kincaid	Cincinnati, O.

The Society adjourned to meet at 2 o'clock P. M., on the third Tuesday in May, 1867, at Indianapolis.

M. H. HARDING, M. D.,
President.

WM. F. HARVEY, M. D.,
Secretary.

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REPORT OF THE COMMITTEE ON PRIZE ESSAY.

The Indiana State Medical Society, through its Committee on Prize Essays, offers a premium of the value of one hundred dollars, to the physician resident within the State, who shall present the best, and an acceptable, dissertation upon any subject of professional interest.

Competitors for the prize must send their essays to Jas. F. Hibberd, M. D., chairman of the committee, Richmond, Ind., free of all expense, on or before the first of April, 1867. Each essay must be accompanied by a sealed packet containing the author's name and address. On the sealed packet must be inscribed some device, motto or sentence, and the same device, motto or sentence must be inscribed on the essay.

The successful dissertation will be the exclusive property of the State Medical Society. The unsuccessful dissertations will be left with the Secretary of the Society, and may be obtained by their respective authors at any time within a year.

JAS. F. HIBBERD, Richmond, Ind.
H. P. AYRES, Fort Wayne, Ind.
WM. B. FLETCHER, Indianapolis.
} *Com. on Prize Essay.*

June, 1866.

Medical Journals, having circulation in Indiana, are requested to notice the foregoing proposition.

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PRESIDENT'S ADDRESS.

BY M. H. HARDING, M. D., LAWRENCEBURG, IND.

The annual return of our social and professional re-union, under circumstances most propitious, and promising a full share of improvement and enjoyment, demands from each member of our association his due proportion of the duties and obligations resulting from its organization. Honored as I have been, in your selection as your presiding officer, it becomes my duty, in conformity with previous usage, to address you upon some subject connected with the profession which deserves our homage and our highest admiration. In accordance with that usage, I propose to address you very briefly upon the Effects of Climate and Temperature upon Health and National Character.

Among the physical agents by which the human frame is modified and influenced, few have a more important relation to the duration of human life, the advancement of civilization, and the various forms of disease incidental to man, than extremes of temperature; and these influences are exerted not only upon the health and development of animal and vegetable structures, but also upon the sensations, the intellectual endowments, and moral emotions of mankind, in the different regions and zones of the world.

The effects of temperature, upon the human frame, vary remarkably, owing to numerous concurrent circumstances, and the extent, rapidity, and frequency of its changes. These often so modify the phenomena of disease as to demand important variations in its treatment. The effects of excessive heat, in its direct operation on the human body, are unfriendly to its highest development in a variety of ways; but it is to its indirect agency, as will hereafter appear, that we are to ascribe some of its most noxious impressions. In passing to the opposite extreme, it may be safely

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pronounced, that there is no evil incident to animal nature that occasions such an extent of distress and annoyance as what is popularly denominated *cold.* In this view of the subject, cold is not only considered as a source of disease, but the privation of a comfort essential to our well being, the absence of which often causes the severest misery. It would be easy, by descending to a few particulars, to demonstrate the operation of this agent in producing a long catalogue of acute and chronic diseases. The memorable conclusion of Sydenham, "that cold destroys a much greater portion of mankind than the combined ravages of war, pestilence and famine," is unhappily verified by universal experience. By recurring to the relative import of the term, and by connecting diminution with variation of temperature, it plainly results that cold may operate injuriously in all parts of the globe; and by still further taking into view the additional impetus it often gives to other morbific causes, we discover another immense range of its destructive influence. It must be familiarly known to this enlightened audience, that the moral and political, as well as the medical, consideration of temperature, has attracted much attention by learned and ingenious writers. And I trust it will not be deemed too wide a departure from the objects of this discourse to notice some of the more prominent opinions which have been held upon this subject. The connection between these several modes of its operation are, indeed, so intimate, that they are calculated, reciprocally, to throw light on one another.

The effects resulting from high, low, and moderate temperature, as felt by man in different parts of the world, are so obscured and modified by other circumstances, that the comparison of their relative advantages and disadvantages, in a general estimate, becomes extremely complicated. It has been commonly held, that in every region of the earth the power of temperature operates with decisive influence in determining the character and condition as well as the diseases of human beings; and that in those countries that approach near to the extremes of heat and cold, this influence is so conspicuous as to attract the attention of the most casual observer. In consequence of this impression, the middle latitudes have been generally supposed to enjoy the blessings of nature in an unrivaled degree. Whether we consider man, says an admired historian, merely as an animal, or as a being endowed with rational powers, which fit him for activity and speculation, we shall find that he has uniformly attained the greatest perfection of which his nature is capable in the temperate regions of the globe. There his constitution is most vigorous; his organs most acute; his form most beautiful. There, too, he possesses a superior extent of capacity, greater fertility of imagination, more enterprise, and a sensibility of heart that gives birth to passions not only ardent, but persevering. In this favorite situation he has

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displayed the utmost efforts of his genius, in literature, in policy, in war, in commerce, and in all the arts which improve or embellish life.

It is chiefly with respect to temperature that so much stress has been laid on those imaginary lands or *zones* encircling the earth, and denominated *climates*.

These divisions of the globe, parallel to the equator, have been invented by geographers to designate the difference in the length of days and nights, degrees of temperature, and some other effects that result from the inclination or obliquity of the sphere; but the term possesses a much wider signification in medicine, and is more commonly applied to the soil, condition of the country, surface elevation, and position of a country, in connection with the general states of the atmosphere, influencing the health and development of the human species and the higher races of the animal kingdom. In this sense we shall employ the term. If we admit the political and moral influence of climate and temperature to the degree contended for by many, we shall be the more disposed to yield our assent to the opinion of its vast power in modifying the health and organization, not only of individuals, but races. Montesquieu seems to have comprehended very fully the influence of those climacteric agencies in modifying human, as well as animal, organization, ascribing to them the principal differences that have been found in the manners, characters, governments, laws, and religions of different nations. In cold countries, he observes, people are more vigorous, and this superiority of strength produces many striking effects, such as intrepidity and conscious elevation, which banish revenge and cruelty; it also produces a higher sense of independence and security, and while it cherishes frankness and confidence, it repels artifice and cunning. On the other hand, he contends that the inhabitants of warm countries are more feeble and timorous, and possess more exquisite sensibility; and he believes, indeed, that the heat of climate may be so excessive as to deprive the body of all vigor and activity. From the body this feebleness is communicated to the mind; active curiosity, daring enterprise, generous sentiments, are all extinguished; the inclinations become debased and passive; indolence becomes the highest enjoyment; no punishment becomes so intolerable as active exercise of the mind or body; and slavery, with all its humiliation, is more supportable than the vigorous action of mental or physical powers necessary to the maintenance of a state of independence and freedom. The facts respecting the Indian tribes of high southern latitudes, and the effeminacy of southern Asiatic nations, are supposed to fully establish this principle. In his career of generalization, Montesquieu, in his "Spirit of Laws," ascribes these effects to the influence of heat, and lays it down as an axiom, that the inhabitants must necessarily be indolent, inert in body, and, consequently, inert in mind and char-

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acter. He even proceeds a step further, and maintains that unlimited monarchies grow out of the condition of these nations; and considers despotism as the effect of that supineness generated by heat. He concludes that despotism is the natural government of such countries, and as unavoidable as any of the other consequences of the climate in which they live.

In the past history of the earth, (I quote from Draper's Civil Policy of America,) there have been slow variations in the vegetable and animal kingdoms, caused by slow variations in the condition of external nature. Physical influences have modified organic forms; but now, if we rise to a higher point of view, and examine what has been the cause of the changes in those controlling influences themselves, what is it that we see? The operation of universal law!. The decline of the earth's heat—an effect which has carried with it the most prodigious modifications, both among living and lifeless things. That decline has been going on under a resistless operation of a law capable of mathematical expression—a law absolutely independent, free from all possibility of change. Thus, when we pursue the scientific investigation of facts to the last accessible point, there uniformly emerges the conception, the idea of law—law ever enduring—exhibiting no variation, but so operating that out of the invariable and eternal, the changeable and the perishable spring forth.

We gather, therefore, a most important lesson from inquiries respecting the origin, maintenance, distribution, and extinction of animals and plants—their balancing one against another—from the variations of aspect and forms of an individual man, as determined by climate; from his social state, whether in repose or in motion; from secular variations of his opinions, and the gradual dominion of reason. The lesson is, that the government of the world is accomplished by immutable law. Such a conception commends itself to the intellect of man by its grandeur. It makes him discern the eternal through the vanishing of present events, and through the shadows of time. From the life, the pleasures, the sufferings of humanity, it points to the impassive; from our wishes, wants, and woes, to the inexorable. In whatever direction we look, we may, therefore, expect to find proofs of the dominion of law.

Notwithstanding the speciousness of this doctrine of the influence of heat, enforced with eloquence by intelligent men, it seems to be liable to insurmountable objections, and has been often opposed by great force of facts and argument.

The lessons of history, it has been said, appear to give little countenance to this doctrine. The ambition and conquests of the Assyrians, for more than 500 years, agitated a great part of the Asiatic world. That vast empire was, in its turn, overthrown by the Medes. The Persians, under Cyrus, within the space of thirty years, extended their conquests and ravages from

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the Indies to the Mediterranean. Achievments like these, surely, give no proof of bodily or mental enervation. But if the proofs of energy, derived from war and conquest, should not be deemed sufficient to efface our impressions of the enervating effects of heat, let us behold the evidences of vigor in other departments of exertion and enterprise.

The Phœnicians, for many centuries, were in possession of the commerce of the whole ancient world. The ruins of Palmyra are stupendous monuments of the industry, magnificence, and taste of that celebrated city. If these are marks of indolence, where shall we look for activity and vigor? But if the influence of climate be so predominant and irresistable, why, in the same countries where so much energy was formerly displayed, do we now find such degrading indolence—such universal degeneracy? Why are the modern Greeks so much debased amidst the ruins of Athens and Sparta—in the fields of Marathon and Thermopylæ? If indolence be peculiar to southern countries, how shall we account for a Carthage in Africa—a Rome in Italy—the Buccaneers in the West Indies—Palenque and Uxmal in South America? These are some of the facts and reasoning by which the doctrine of Montesquieu has been often assailed and refuted. They seem to be entirely unanswerable; for, however disposed we may be to admit the power of heat over the bodies and minds of men, it must be allowed that moral and political causes, forms of government, and social institutions affect the character of nations as well as individuals still more decisively than the influence of climate. While declining to express any opinion as to the unity of the human race—that is, its derivation from one primordial pair—we must, in view of the numerous and modified forms of men now dispersed throughout every portion of the earth, assign almost paramount control to climate and modes of life; but the conclusions to which we are compelled, when the question is broadly and definitely stated, would doubtless be very reluctantly received.

It may be asserted that all action, whether of body or mind, has its chief source in our necessities, and progressively augments as they increase. The facility for obtaining an abundance of food depends much upon the soil, hence it results that the nature of the soil has a real influence on the physical and mental activity of its inhabitants; and we must perceive, that in the social as well as in the savage state, a country in which the means of subsistence are somewhat difficult to be procured, will always have active and industrious inhabitants; while in another, where nature has lavished, with an unsparing hand, everything necessary to the subsistence of man, the people will be indolent and inactive. It is not, therefore, as inhabitants of hot, but as inhabitants of rich, countries that nations are inclined to indolence. In applying these remarks to the northern and southern States of this Union, it will not be deemed invidious to remark, that it would seem probable, even

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if observation did not attest the fact, that the ruggedness and sterility of the soil in the North, aided by the degree and duration of cold, must produce a race of hardy, active, and industrious inhabitants, while the exuberant fertility and rich and varied productions of the South, with the additional advantages of a milder climate, must cherish the propensity to indolence and effeminacy.

It has long been a matter of very great dispute whether the differences, intellectual and physical, presented by the various races of men, have arisen from the slow and imperceptible operation of climate or other external agencies, or have been originally impressed by the Creator upon the species. The evidence and arguments connected with this subject do not fall within the province of this essay; but while such original distinctions of race are altogether hypothetical, the discrepancies which are caused by differences of climate, food, and soil, are capable of satisfactory explanation; and when understood, will be found to clear up many of the difficulties which still obscure the study of history. It is evident that by the combinations of these agents originated the most important consequences, in regard to the general organization of society; and from them have resulted many of the large and conspicuous differences between nations and races of men, which are often ascribed to some fundamental difference in the various races into which mankind is divided. It is of importance to the practical physician, as well as to any one who entertains philosophic ideas in medicine, that the peculiarities that characterize the different races of men should not be overlooked: thus we infer that the slow and continued operation of a particular climate may actually change the human frame to that state which its indigenous inhabitants present; and the constitution, thus assimilated, is necessarily affected by the external influences to which it is exposed, and the food furnished by the soil of which it is the native. There are, however, certain characteristics, especially those that distinguish the Ethiopian and Mongolian varieties, that a succession of ages has not been sufficient to impart to different races that had migrated to the climate they inhabit, which have, therefore, been imputed by many ethnologists to difference in original conformation. It is evident these great physical powers are dependent on each other in no small degree; the climate of a country, and the food which will be ordinarily grown, are closely connected; while the food itself will be influenced by the soil which produces it, by the elevation or depression of the land, the state of the atmosphere, and the physical geography of the locality. "Scientific Physiology has no better ascertained fact than that man possesses no innate resistance to change." The moment he leaves his accustomed place of abode to encounter new physical conditions and altered modes of life, that moment his structure commences slowly to change. It may take many generations before an equilibrium is reached. Then his countenance, his complexion, his hair, testify to the

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transformation that has been going on. 'Of all the great social improvements,' (I quote from Buckle's History of Civilization,) 'resulting, among a people, from their climate, food, and soil, the accumulation of wealth must be the first, because without that there can neither be that time nor taste for the acquisition of that knowledge on which the progress of civilization depends.' Among a people entirely ignorant, the rapidity with which wealth is created, will be regulated solely by the physical productions of their country, and the energy and regularity with which labor is conducted—the latter being entirely dependent on the influence of climate. If the heat is intense, men will be unfitted and indisposed to that active industry which, in a milder clime, they might willingly have exerted. It is well known, too, that climate influences not only labor, by enervating the laborer, or by invigorating him, but also by the effect it produces upon the regularity of his habits. People living in a very northern latitude, never possess that steady and unflinching industry that distinguish the inhabitants of temperate regions; extremely low temperature, and sometimes deficiency of solar light, renders it impossible to continue the usual out-door employments. The working classes are rendered more prone to desultory habits; the chain of their industry is broken; and they lose that impetus and force of character which uniform and uninterrupted practice never fails to give. Hence there arises a natural character more unstable and capricious than that possessed by a people whose climate permits the regular exercise of their ordinary industry. This fact is so clear that it seems superfluous to demonstrate it further by examples; a slight examination of the comparative difference in governments, manners, religions, and laws of countries, situated in different latitudes, will abundantly prove this proposition.

It must be admitted that the climate, in the broadest sense of the term, including not only the temperature of countries, and the phenomena upon which depend the distribution of heat, but all the modifications of the atmosphere, which sensibly affect our organs, as changes of electrical tension, and emanations from the soil, exert a powerful influence, not only upon the development and health of the vegetable and animal kingdoms, but also upon the sensations, the intellectual endowments and the moral emotions of mankind, and consequently on their rapid advance in civilization and refinement in the different regions and zones of the world. To these potent influences, Mr. Buckle (History of Civilization) has ingeniously added another agent, perhaps no less effective in modifying man in his social and physical development, viz.: the aspect of nature, or those appearances which, though presented chiefly to the sight, have, through the medium of that or the other senses, directed the association of ideas, in different countries, to different habits of natural thought. He maintains that it produces its principal results by exciting the imagination, and by suggesting those innumerable superstí-

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tions which are the great obstacles to advancment in knowledge. And as in the infancy of a people, the power of such superstition is supreme, it has happened that the various aspects of nature have caused corresponding varieties in the popular character; and have imparted to the national religion peculiarities which, under certain circumstances, it is impossible to efface. It would be exceedingly interesting to elucidate this branch of the subject, but time and space forbid, and we must dismiss it with this brief notice.

Degrees of latitude only approximate the degrees of heat or cold of any locality or climate. The various meteorological changes that particular localities or countries are subject to with so much regularity, are as much the result of natural causes as those regulating the motions of the solar system, producing invariably changes of temperature. The elevation above the ocean surface; the prevalent currents of wind; mountain ranges; and the declivity and configuration of the surface of the country, are all influential in modifying its temperature, and increasing or lessening its adaptation to vegetable or animal development—establishing the existence of isothermal lines that are vastly more reliable in estimating the salubrity, or climate of a country, than mere degrees of latitude.

Very much that is obscure and imperfectly understood, as yet, of the causation of disease, and especially disease of an endemic or epidemic character, will be connected and explained by a more familiar acquaintance with laws governing climatic and meteorological phenomena. This broad and comparatively uncultivated domain belongs, legitimately, to the medical profession; and doubtless some patient and philosophic laborer in this important field of inquiry, will yet make discoveries, as to the cause and prevention of disease, that will shed lustre on our profession, and entitle the discoverer to the distinction of being classed with the greatest benefactors of the human family.

In applying the general principles, thus briefly noticed, to the physical condition, social progress, moral and intellectual advancement, and present and prospective aspect of political institutions, of the people of the United States, we may safely, and without undue presumption, predict for them a steady and progressive advancement, in all the requisites for a high degree of civilization, and all the material elements of improvement, in the institutions which embellish and adorn society. Extending from the Atlantic to the Pacific oceans, and from the lakes of its northern extreme, to the Gulf of Mexico, it comprises within its vast limits a variety of climate and physical aspects such as has fallen to the lot of no other portion of the globe: embracing a vast area of soil producing in abundance every variety of food; a climate suited to the physical development and sustenance of a boundless population; and an accumulation of wealth sufficient to develop its agricultural, commercial, and mineral resources: and with a vast

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system of intercommunication, afforded by its natural and artificial means of locomotion: and, above all, with a system of government affording to all political equality and protection, we may look forward to a career of prosperity as a great, an active, and a homogeneous people, that will render them memorable in the annals of the human race—

> "For here, before almighty law,
> High birth, high place, with pious awe
> In reverent homage bend.
> Here man's free spirit unconstrained,
> Exults in man's best rights maintained;
> Rights, which, by ancient valor gained,
> From age to age descend."

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CHOLAGOGUES

AND THE INDICATIONS FOR THEIR USE.

BY V. KERSEY, M. D., RICHMOND, IND.

Let it be remembered that this paper owes its origin to a transient discussion in this Society, last year, of some of the views advanced in a contribution, by Dr. James F. Hibberd, on the Liver, since published in our transactions.

The acquisition of truth is the object of all scientific pursuit. In some departments of knowledge, the first step in the process is the perception of certain self-evident propositions; and each subsequent step is the perception of a necessary deduction therefrom. Hence certain sciences, as mathematics, astronomy.

In most departments of human knowledge, this mode of reaching truth is inapplicable. We are not entirely certain as to the premises; we are unable to estimate every element at its absolute value: thus our inductions fall short of demonstration. Hence the uncertain sciences, chemistry, medicine.

As, in mathematics, one erroneous assumption vitiates all subsequent conclusions, so, in medicine, the impossibility of clearing the premises, precludes positive knowledge. In medicine, doubtless, as in astronomy, cause and effect hold their immutable relations. There is no infidelity in nature. The trouble lies in our finite capacity—a trouble, obviously, never to be fully overcome. And hence, though medical science continues to engross its full share of the talent and culture of the race—though it reaches a precision and utility beyond the most benevolent dream of this age—it must still remain among the uncertain sciences. It is the special domain of judgment, requiring and

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prompting the most thorough development of this high faculty of our nature.

It is an easy matter, in our profession, to fall into error. Perhaps most men do, either in the way of credulity or scepticism. It is a fundamental, and not uncommon error, on the one hand, to ascribe all favorable progress in disease to the influence of medicine, and to reckon every *recovery* as a *cure*. On the other hand, to refer all this favorable progress to the conservative power of life, often pitted alike against both disease and the Doctor, is an error not less essential, though perhaps less common.

It were well for medical men to consider how much of what is called medical science lies confessedly outside the limits of demonstrative truth, and how much, that we accept as true beyond probation, is wanting in those broad grounds of conclusive support, which should underlie every principle of our art. The stability of the profession is not secured by adherence to error, however long cherished, but rather by the cultivation of an attitude favorable to the acceptance of all truth.

On the other hand, how small a part of what we know beyond a doubt admits of proof beyond a cavil. The occurrences of every day illustrate the impossibility of demonstrating even the most unquestionable facts of medical experience. It can not be shown beyond a cavil that the physician ever cures disease, or even prolongs life, yet these propositions rest on a basis secure as those of Euclid.

Thus, then, not without some appreciation of the difficulties in our path, let us attempt an elucidation of the biliary function of the liver, in the light of such trustworthy evidence as the present state of scientific research on the subject has furnished.

The circulation of the liver is peculiar, and important in its bearings, both physiological and pathological. The supply of arterial blood is remarkably small, the hepatic branch of the cœliac axis being much less than the splenic, although the liver is six or eight times as large as the spleen; besides, the former vessel gives off considerable branches to the pancreas, duodenum, stomach and omentum. It is simply the nutrient vessel of the liver, supplying the blood-vessels, hepatic ducts, gall bladder, glandular structure and capsule of Glisson. All the blood conveyed into the liver by the hepatic artery, even that nourishing the hepatic veins, is ultimately transferred to the portal veins, with which the artery inosculates freely.

The portal system of vessels originates in capillaries, which receive the blood of the spleen, pancreas, stomach, mesentery and whole tract of intestines, converge toward the transverse fissure of the liver, where they form a single trunk which enters the liver, divides, sub-divides, and ramifies through portal canals to every part of the gland, until it is again reduced to capillaries, a plexus of which, with the enclosed plexus of biliary ducts, and the

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intervening cells, forms a lobule of the liver, and a miniature representation of the entire biliary apparatus. This arrangement of the ultimate capillaries of the portal veins upon the acini of the liver, has its analogue in the vascular network of the pulmonary arteries which encloses each lobule of the lungs; neither is the analogy confined to the anatomical structure of the parts, for in both the blood is subjected to a process of depuration essential to life.

In man the liver is placed between the alimentary canal, the great receptacle of nutriment, on the one hand, and the mass of circulating blood on the other.

According to the observations of Bidder and Schmidt, the quantity of fluids poured into the alimentary canal of a healthy and well-fed man, in twenty-four hours, by vascular exosmosis and glandular action, is about twenty-four pounds; to which, if we add seven pounds of ingesta, solid and fluid, we have an aggregate of thirty-one pounds. The same observers estimate the daily loss in fæces at five ounces, and the quantity of fluids taken up by the lacteals at six and a half pounds. From these data, which are generally esteemed nearly accurate, it is apparent that about twenty-four pounds of intestinal fluids and ingesta are daily absorbed by the capillary radicles of the portal veins, mixed with the portal blood, and submitted to the action of the biliary apparatus of the liver.

The mean specific gravity of normal human blood is 1,055. That of its fresh serum 1,028. The specific gravity of fresh human bile is about that of urine, ranging from 1,010 to 1,020.—*Schmidt.*

One hundred parts of portal blood contain thirty-four parts of serum, while the same quantity of blood from the hepatic vein contains only fifteen parts of serum.—*Lehmann.*

The blood of the portal veins contains about 100 per cent. more of the fats, and about 23 per cent. more cholesterin than the blood of the hepatic vein.—*Lehmann; Flint, jr.*

The cells of portal blood contain nearly 100 per cent. more hæmatin than those of the hepatic venous blood.—*Lehmann.*

The abundance of water, the fats, cholesterin, and hæmatin in portal blood compared with hepatic venous blood; the low specific gravity of bile compared with blood, or even serum; and the abundance of the fatty acids, cholesterin, and modified hæmatin which bile contains, indicates too clearly to be overlooked, the portal blood as the source of this fluid.

From the portal blood, containing as it does a large amount of intestinal fluids and ingesta, there is eliminated daily about three pounds of fluid bile.—*Bidder; Schmidt; Nasse; Chambers.*

Although the flow of bile is constant, it is greatly increased during the hours of active digestion.—*Bidder; Schmidt.*

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The liberal use of water increases the flow of fluid bile, and organic solids, though not of the biliary salts.—*Bidder; Schmidt; Nasse.*

When the digestive function is active, and the normal quantity of food is increased, we have a corresponding increase in the flow of bile.—*Bidder; Schmidt; Nasse.*

When digestion is suspended by disease, or food is withheld, the secretion of bile is soon reduced to less than one-fourth the normal quantity.—*Bidder; Schmidt; Nasse.*

The daily diminution of the biliary secretion in fasting animals, occurs in almost the same ratio as the loss of fat in the body.—*Bidder.*

From the tenth day of inanition, in the case of a cat that died on the eighteenth day, all the bile that was secreted passed into the fæces.—*Bidder; Schmidt.*

Many of the above cited facts, physiological and pathological, point directly to the *portal blood* as the source of bile. This idea is fully sustained by nearly all eminent microscopical anatomists. It is believed to have been demonstrated by Kiernan, Lereboullet, Beale, and right recently by the well directed and persevering labors of our sagacious countryman, Prof. Salisbury. On the other hand, Morel teaches in his "Manual of Histology," and Dr. Hibberd affirms in his "paper on the liver," that "the hepatic artery *only* must furnish the ingredients for the bile."

Fresh human bile is not always precisely the same. It consists, nevertheless, at all times, of water, holding in solution, near a score of proximate principles, and in suspension, several insoluble organic substances. Food, drinks, diseases and medicines, modify materially, not only the aggregate amount of the secretion, but its specific gravity, and the relative proportion of its constituents.

A chemist and physiologist, still in active life, second perhaps to few living men in extent, variety, and accuracy of knowledge, Prof. C. G. Lehmann, in his work on physiological chemistry, says, "The most distinguished chemists of our time, founding their views on the most exact experiments, have been led to perfectly different results concerning the constitution of the bile. It must be recollected that the bile undergoes decomposition with extraordinary rapidity. Scarcely any chemist assumes that he has employed perfectly undecomposed bile in his analyses."

I shall consider only some of the more important proximate principles of human bile.

Glycocholic acid, (associated, in the lower animals, with taurocholic acid, and both frequently called cholic, cholalic, and choloidic acids, the resinous biliary acids, the biliary acids proper, and the biliary substances proper,) in the form of a soda salt, constitutes a large proportion of the solid material of human bile, in a healthy and well-fed subject. It is the only substance af-

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fording the characteristic reaction with Pettenkofer's test, which, indeed, is no test for any other constituent of human bile, elementary or proximate. This test is, however, almost exclusively relied on to determine the presence or absence of bile. Hence, it is apparent that negative results indicate the absence of this constituent only, from any specimen in question.

There are many cogent reasons for the conjecture that glycocholate of soda consists, to a great extent, of partially assimilated aliment, derived in well-fed subjects, largely, from the nutriment taken up by the radicles of the portal veins: that it assumes this form under the action of the liver, as one of the stages of the complex process of assimilation: that in perfect health, and under appropriate regimen, it is, to a great extent, resorbed from the intestine, and appropriated to the purposes of nutrition: that it is not poured out during the process of digestion, simply to promote digestion, as we are often told, but as one of the results of that process, namely: a product of hepatic digestion: that the amount secreted depends upon two contingencies, the supply of aliment, and the functional activity of the organs concerned. The absence of glycocholate of soda, as such, from the portal blood and lacteal vessels, although it is known to be largely reabsorbed, affords direct support to this conjecture; for stability of chemical constitution is not compatible with just ideas of aliment. We see food constantly changing its form, as well as its chemical constitution, at every stage of assimilation, until it becomes living tissue. And, without this progressive change, we have no nutrition.

Lehmann says, on this subject, a portion of the organic matters must pass through the stage of biliary formation, and then return from the intestine into the blood, in order that it may be applied to further purposes.

The chief biliary constituents that are absorbed are the soluble salts, (glycocholate and taurocholate of soda,) and the cholic acid, liberated from its adjunct.—*Lehmann.*

In the large intestines, and in the solid excrement, I have invariably found a substance, soluble in ether, and in such small quantity that, after as correct an estimate as it was possible to institute, it could not be assumed that it represented all the bile that had been effused from the liver into the duodenum; but we are rather led to Liebig's view, that a large part of the bile is again absorbed in the course of the intestinal canal.—*Lehmann.*

To the same end we might quote many other able physiologists of our own time, as well as Boerhaave, and other distinguished names of the last century, but it is deemed unnecessary.

Another kind of evidence, not less conclusive of the resorption and nutritive qualities of a portion of the bile, is derived from the well authenticated fact that animals with biliary fistulæ, through which the entire secretion from the liver is withdrawn from the body, have ravenous appetites; and, notwith-

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standing an abundant supply of nutritious food, they soon die apparently of simple starvation.—*Bidder; Schmidt; Lehmann; Nasse; Dalton.* Also from the fact that in the advanced stage of inanition the bile becomes entirely excrementitious.—*Bidder; Schmidt.*

Bidder and Schmidt have attempted to determine the proportion of bile thus resorbed, by comparing the quantity of sulphur discharged in the fæces with the whole quantity poured out by the liver. They find in the fæces one-eighth to one-fifteenth only of the sulphur discharged by the liver. Hence they conclude that seven-eighths or even fourteen-fifteenths of the bile discharged by the liver is resorbed from the intestines.

It should, however, be borne in mind that this calculation applies to but one of the proximate principles of bile, namely: taurocholic acid, of which alone sulphur is a component element. And hence, according to this showing, which by the way is the chief argument of those who maintain that bile is not excrementitious, *a considerable portion of taurocholic acid is constantly discharged*, and the other elements of bile may be *wholly* discharged with the fæces.

Cholesterin has been recently made the subject of an elaborate series of well-devised experiments; unfolding its entire history; tracing it to its source in the vital decay of nervous matter; fixing its status as an excrementitious element of bile; demonstrating its accumulation in the blood in torpid conditions of the liver; and showing that, in such circumstances, it has an important agency in the production of fatal disease.

For the satisfactory elucidation of this hitherto obscure subject, the profession is exclusively indebted to Prof. A. Flint, jr., of New York.

Stearin, margarin and olein, as well as cholesterin, are always found in the blood; are constantly present in the bile in the form of soda and potash salts; and, under some modification, are always present in the excrements. Like the former, they accumulate in the blood when the function of the liver is suspended; and, like it, are doubtless capable, under such circumstances, of producing disease.

Stearic, margaric and oleic acids occur in the blood, in the bile, and in the excrements.—*Lehmann.*

In acute diseases generally, there is a decided augmentation of fats and cholesterin in the blood.—*Lehmann.*

The blood of drunkards, with granular disease of the liver, presents large accumulations of free fat.—*Lehmann.*

In two patients with liver affection, with excrements pale, almost grayish white, the blood was largely charged with fats.—*Lehmann.*

In chronic diseases of the liver, such as icterus, cirrhosis, &c., the fats and cholesterin are especially increased in the blood.—*Lassaigne; Becquerel; Rodier.*

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Traill and Lecanu found the blood extremely rich in fats in inflammation of the liver.

Cholepyrrhin, biliverdin, bilepigment, a well known element of bile, has never been successfully analyzed, nor the quantity excreted accurately ascertained. This substance is derived from hæmatin, the coloring matter of blood-corpuscles, in the process of vital decay, and affords another illustration of hepatic depuration. When, however, this function is suspended, the skin, acting vicariously, attempts the elimination of the effete hæmatin, and converts it into hæmatoidin, a substance, so far as yet known, chemically identical with biliverdin. And thus, as maintained by Speir, the various tints of jaundice are produced by hæmatoidin, and not by resorbed biliverdin as heretofore generally supposed.

Bilepigment is never entirely absent from the excrements, except in those rare cases of icterus, which are accompanied with a complete stoppage of the biliary secretion.—*Lehmann.*

Bilepigment mixes with the contents of the intestines, becomes very rapidly modified, and ceases to present the usual reaction with nitric acid. The change which it here undergoes appears to be the same which we can induce artificially by nitric acid. It is in this form that it occurs in the solid excrements. In diarrhea unchanged bilepigment occurs in the dejections.—*Lehmann.*

The analyses of Lehmann, Simon, Becquerel, Rodier, Traill, Lecanu, Kölliker, Flint, Speir, Antisell and others, leave no room to doubt that stearic, margaric and oleic acids, cholesterin and the bile pigments, are excrementitous biliary substances; removed from the blood by the action of the liver; and discharged in the fæces, more or less altered, according to the rapidity with which they pass through the alimentary canal.

In the physiological state, under full diet, these strictly excrementitious elements of bile bear, to the so-called biliary acids proper, about the proportion of one to six and a half. But in pathological states, and when food is withheld, this proportion is no longer maintained. It may, as we have seen, be reversed. Nay more, the formation of recrementitious bile may be entirely suspended.

This condition is well illustrated by Prof. Antisell, *American Journal Medical Science*, Jan., 1864, where he gives the results of his examination by dialyses, the microscope, and chemical tests, of the bile taken from the gall-bladders of ten men who died of chronic diarrhea. He says: "This mode of examining bile by dialysis, if supported by other observers, must lead us to reject the present view of the function of the liver as a *secretion*, and of the nature and sources of bile." He is "led to believe the office of the liver is to *purify the portal blood*, by removing from it the *cholesterin*, *fatty acids* and *pigments* by means of the hepatic cells."

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A very similar condition of the biliary function doubtless obtains in all cases where digestion is wholly suspended, either by disease or starvation, as well as in the state of hybernation and in fœtal life, before the function of the stomach is called into action. I am aware that Prof. Dalton says, "the meconium is not composed to any appreciable extent of the secretions of the liver." But on the other hand Prof. Lehmann says, "the small intestines of the human fœtus, even as early as the sixth month, always contain a bright yellow mass, yielding, on analysis, margaric and oleic acids, saponifiable fats, bile pigments, epithelial structures, and obscure traces of the so-called biliary acids proper."

Science has thus distinctly indicated the source of the meconium, and the substances of which it is composed. Notwithstanding, as it exists in the large intestines at full term, the proximate elements are so changed that they do not readily admit of recognition by the usual chemical tests. It is, however, easy to detect in the meconium at birth, after months of retention in the large intestines, cholesterin, the fats, epithelial structures, mucous corpuscles, and all these colored blackish green, by an abundance of altered pigment, which as might well be foreseen, does not afford the reaction of cholepyrrhin with nitric acid.—*Lehmann.*

Thus starvation, chronic diarrhea, and the fœtal state, as well as that of hybernation, alike afford apt illustration of the depuratory function of the liver. They do not, however, disprove a digestive and an assimilative function of the same organ, the proof of which rests on equally conclusive observations upon the active adult in the physiological state.

In our attempt to show that a portion of the so-called biliary acids proper is resorbed, we get still more conclusive evidence that another and considerable portion of these same acids is constantly discharged with the fæces. But inasmuch as this fact is strongly controverted by some, it seems right to present more fully the results of analytical research on the subject.

The bile becomes gradually decomposed in the course of the alimentary canal; the conjugated acids, (glycocholic and taurocholic,) breaking up and forming choloidic acid, which becomes converted into dyslysin, a substance that may be traced into the rectum and fæces.—*Lehmann.*

In the alcoholic extract of the contents of the large and small intestines *we find biliary constituents.—Lehmann.*

In the ether extract we find substances which give the well-known biliary reaction with sugar and sulphuric acid. The further we descend in the intestinal canal, the less of these resinous acids do we find in the alcoholic extract, but a comparatively larger amount passes into the ethereal extract.—*Lehmann.*

Recounting the difficulties to be overcome in the analysis of normal solid

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fæces, the author says decomposed bilepigment and putrid biliary matter adhere to everything.—*Lehmann.*

Valentin says, the offensive odor of healthy fæces is much more dependent on decomposed constituents of bile than on the remains of food.

In jaundice, with occlusion of the biliary duct, the excrements are of a dirty, whitish-gray color and putrid odor.—*Lehmann.*

Wehsarg, who was extensively engaged in analytical observations on normal fæces, says, the color varies with the food: on mixed diet they are of a yellowish brown tint; on flesh diet they are much darker; and on milk diet quite yellow.

In all cases in which the food passes more rapidly than usual through the alimentary canal, a large quantity of undecomposed bile is always present. Hence this is the case after purgatives and in diarrhea, as Pettenkofer himself proved.—*Lehmann.*

Taurin, one of the elements of taurocholic acid, is to be found in the whole course of the alimentary canal, and in the solid fæces.—*Lehmann.*

Kuhn has found that cholalic acid, choloidic acid, and dyslysin exist normally in dog's fæces, and in such large quantities that he concludes that almost none of the bile acids are resorbed from the intestines, but that they pass off, for the most part, with the excrements.

We meet with large quantities of grass-green bile in the matters vomited in inflammatory affections, especially abdominal and cerebral, which color is induced by the action of the free acid of the gastric juice on the bile pigment.—*Lehmann.*

Polycholia, or abundant purging of bile, constitutes the affection known as icterus neonatorum.—*Lehmann.*

The bright yellow semi-fluid excrements of children at the breast contain fats, casein, cholepyrrhin and biliary acids.—*Lehmann.*

A green coloration of the excrements is almost exclusively limited to true polycholia; in these cases the cholepyrrhin is converted, in consequence of the predominance of free acid, into biliverdin. The application of tests for biliverdin, and the biliary salts, leaves no doubt regarding the abundant existence of almost unchanged bile in these stools.—*Lehmann.*

I have presented above some of the results of an elaborate series of analyses of fæcal matter, conducted wholly in the interests of scientific truth, and trustworthy as the science of chemistry itself. It may now be proper to introduce a sentence or two from the paper in discussion at our meeting last year.

"In health the color of the dejections from the bowels are (is?) not in any degree dependent on the presence or absence of bile. The same is true in disease, and even under the administration of the so-called cholagogues."—*Hibberd.*

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"The bile is poured into the intestine within a few inches of the stomach, and at once mingles with the food or other contents of the canal; loses its distinctive character, leaving the progressive contents of the small intestines without color until they pass the ileo-cæcal valve. Indeed the color, as well as the other characteristics of the fæces, do (does?) not show themselves (itself?) until the mass begins to accumulate in the colon."—*Hibberd.*

Prof. Dalton, of New York, an author and observer of no mean standing, fed a number of dogs with fresh meat; killed them at different intervals after the meals; placed ligatures on the intestines at various points; and carefully examined the contents of each section. These results *all concur in showing* "that the entire quantity of the intestinal contents diminishes, and its consistency increases, as we approach the ileo-cæcal valve; and at the same time the color changes from a *light yellow* to a *dark bronze* or *blackish green*, which is *always strongly pronounced in the last quarter of the small intestines.*" Dalton's Physiology, 3d Ed.

Again, Dr. Hibberd states in "the paper on the liver," that "in man the green color of the stools, after the use of purging mercurials, is due to the presence of sub-sulphide of mercury."

I suppose every intelligent physician of much experience has often carefully observed the appearance of *bile*, as it exists in the hepatic ducts, in the gall-bladder, in the intestines, in vomited fluids, and in fæces; and all this in cases where mercury has been used, as well as where it has not been used; has witnessed the various shades of yellow, green and brown which it assumes; thinks himself familiar with the appearance of bile—fresh, acidified, oxidized and decomposed; has seen the like appearances arise in cases of jaundice, attended with clay-colored stools, on the use of mercury; has hailed them as indications that the liver had resumed its function; and has been confirmed in this impression by the declining icterus and restored health of his patient. In other instances of icterus, with clay-colored stools, he has given mercury time and again; has kept it up day after day; the stools have continued clay-colored throughout; the restoration of the function of the liver and the *coloration* of the fæces with *sub-sulphide of mercury*, being alike obstinately out of his reach; and withal his patient has remained icterode and sick. It is natural, under such experience, to conclude that mercurials promote the functional activity of a torpid liver; that they favor the excretion of bile; in a word, that they act as a *cholagogue*, and not as a simple *pigment*, on the contents of the alimentary canal. But inasmuch as no amount of experience however striking and conclusive can possibly settle this question beyond a cavil, we are fortunate in being able to refer its decision to positive chemical experiment.

Lehmann says every one is acquainted with the grass-green, pulpy stools which so frequently follow the administration of calomel—about which *color-*

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ation there have been more controversial discussions than experiments. After calomel has been taken, we always find mercury in the stools, whether they be green, black or natural in color. The dark color of the sulphide of mercury may give rise to a light green color with animal substances, and especially with yellow bile pigment. But notwithstanding this fact, we should not deny the presence of almost unchanged *bile in calomel stools;* for we may with facility recognise the presence of bile pigment by nitric acid, and the resinous biliary acids by Pettenkofer's test.

Thus, then, the experience of the profession is fully vindicated. The universal conviction derived from observation, that light-colored fæces indicate functional torpor of the liver is no *myth*, but a *scientific verity.* The bronze color of healthy fæcal matter is due to the presence of decomposed *bile.* Light colored stools contain little or no bile. And they indicate with certainty that little or no bile is poured out by the liver. The characteristic high-colored fæces, yellow green and brown, that so constantly follow mercurial purgatives, owe their color to the presence of *bile.* If these colors are unnaturally intensified, there is an abnormal abundance of *bile.* If they are bright-yellow, *bile*, without acidification or oxidation, has made them so. If they are bright-green, a free acid has changed the cholepyrrhin into biliverdin, and made them so. If they are brown, *bile*, oxidized and partially decomposed, has made them so. That the use of mercury is followed, with great uniformity, by characteristic high-colored fæces, admits of no doubt. Those who utterly deny the presence of bile in the fæces, testify to this fact and attempt its explanation. Hence, in showing that these characteristic fæces owe their color to the presence of *bile, we show that mercury is a cholagogue;* not upon the testimony of one man, nor of one thousand men, but of the entire medical profession; not of the adherents of the doctrine alone, but of those who utterly reject the idea of a cholagogue property in medicine.

We might perhaps safely rest this question right here without further evidence or illustration, but I solicit your indulgence while I present a few other facts of like bearing.

Dr. Hanfield Jones administered medicines to animals, killing them shortly after, to determine their effects on the liver.

Mercurials and manganese produced a very great sanguineous congestion of the liver, and increased the yellow matter in its cells.

Nitro-muriatic acid caused a great flow of bile *per anum* during life. Aloes and rhubarb alike flow into the intestine; but the yellow matter in the liver cells was not increased, and the liver was pale.

It is extremely probable that had the animals that took the mercury and manganese been permitted to live a little longer, they too would have had a

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great flow of bile *per anum* during life; and, on slaughter, would have presented pale livers, divested of yellow matter in the cells.

As the case stands, it appears, at least, that nitro-muriatic acid is a decided cholagogue for cats.

Gmelin found that an extraordinary secretion of bile was produced in animals to which the sulphate of manganese was administered.

In a case where the fæces contained little if any biliary matter, Simon found, after the operation of a large dose of calomel, 21.4 per cent. of the organic principles of bile in the fæces.

Bucheim ascertained, by experiments on dogs with artificial biliary fistulæ, that the use of calomel increased the flow of bile; and on analysis the mercury was found in the bile.

And here it seems right to present another sentiment somewhat sharply discussed at our meeting last year.

"It is one of the marvels of medical science that the cholagogue properties of certain drugs should have been so postively iterated and reiterated, for hundreds of years, by all authors who had occasion to speak of them, and that the declaration should be accepted as truth by every disciple of Esculapius, *and yet the proposition never had a single fact to sustain it.*"—*Hibberd.*

I have presented a few quotations from "the paper on the liver." Their likeness to each other, in some features, is so striking as to enforce a conviction of kindred origin. They are clear enough to preclude misapprehension. They have all the weight that positiveness can give: and they are singularly out of harmony with the truths of science. I might have presented other statements from the same source, like these in every feature, but it was deemed unnecessary.

Dr. C. E. Isaacs took the bile directly from the gall-bladder of a large dog; and applied the usual chemical test for bile, including Pettenkofer's, but was wholly unable to detect its presence, notwithstanding repeated careful trials.

That a chemist and physiologist of the first standing—"the author of the ablest anatomical monograph that our country has yet produced"—should have totally failed, with all the chemical appliances, to detect the presence of bile where bile alone existed, abundant, simple and fresh, is a significant circumstance, and should abate the wonder that others have sometimes failed to find it, by the same mode of research, in solid fæces, where it is known to be greatly modified by many hours of contact, at a high temperature, with the countless substances—mineral and organic; solid, fluid and gaseous—constantly present in the alimentary canal. Nay, more, it stamps at once with discredit the conclusion that bile is never present in the fæces, except where it reacts with Pettenkofer's test.

The effects of suppression of the bile are generally considered to be too

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well known to require testimony. But inasmuch as this fact also is discredited in "the paper on the liver," I here cite the simple declaration of Prof. Dalton on the subject. "The complete suppression of bile produces in the human subject symptoms of poisoning of the nervous system analogous to those which follow the suppression of urine, or the stoppage of respiration, and the patient dies comatose." And yet, strange to say, Prof. D. is prominent among those who maintain that bile is recrementitious, and disappears in its passage through the intestines.

In view of the preceding facts, we can not avoid the conclusion that the biliary function of the liver is two-fold.

This organ prepares, on the one hand, a nutritious substance, so necessary to the well-being of the body that a permanent arrest of the digestive function of the liver interferes very decidedly with nutrition, and usually destroys life apparently by simple starvation. On the other hand, it constantly eliminates certain effete substances so abundant, and so injurious, that the suppression of the depuratory function of the liver is rapidly fatal to life.

The recrementitious elements of bile have their source so directly in the food consumed, that the supply of the latter is a fair index of the quantity of the former.

The recrementitious elements have their source in the products of vital decay—of retrogressive assimilation; and the quantity excreted is the measure of activity of the nutritive function. If, then, we would increase the formation of bile, we must increase the assimilation of food, and intensify the process of nutrition. But perfect health, active exercise and abundant food secure the greatest attainable activity of the function of nutrition. And thus we secure the conditions necessary to the largest possible continuous formation of bile.

While this state obtains, the nervous system will not become embarrassed, nor the blood poisoned, by the accumulated products of vital decay, for they are constantly excreted by the liver, and daily discharged as fæces.

A cathartic, under such circumstances, will disturb digestion and absorption, and hurry out of the system food and recrementitious bile, both of which should be absorbed, to secure the highest nutrition and the most abundant formation of bile. And hence it must curtail the formation of the fluid, although it may temporarily increase the quantity discharged in the fæces.

Calomel will produce all these effects in common with other cathartics. But besides these, "it is known to be a general liquefacient promoting destructive metamorphosis universally, and a consequent increase of solid evacuation from all quarters," and especially of excrementitious bile.—*Chambers.* And yet, in the circumstances indicated, it is by no means probable that the increase from this source would compensate the loss resulting from the unavoidable disturbance of assimilation.

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In an essay on the 'action of mercury upon the liver,' by Jas. L. Brown, M. D., published in the transactions of the Medical Society of the State of New York for 1864, will be found the *inspiration*, perhaps even *more* than the inspiration, of Dr. Hibberd's "paper on the liver."

An important feature of this essay is a careful review of certain experiments by Dr. Scott, in the office of Sir Lionel S. Beale, London, on a dog with biliary fistula. The dog was in good health, weighed 18 pounds, and was well fed on milk, rice and boiled horse-flesh. Four doses of calomel, of three, six, ten and twelve grains, were administered at suitable intervals; and the quantity of bile discharged for two days before and two days after each dose was carefully ascertained. The result was, that a *decrease* of bile, solid and fluid, followed the use of each dose of calomel, though the biliary acids were *increased* in one case.

Dr. Scott says, in the first two cases the decrease was no doubt due somewhat to the diminished quantity of food consumed the day after the calomel was taken.

The dog was in good health, the blood free, of course, from effete matter and his diet full. His smallest dose of calomel is equivalent to half a drachm, and his largest to two drachms, for a man of 160 pounds. It does not seem unnatural that his appetite was poor the day after his medicine was taken. One would expect active vomiting the day it was taken. Perhaps it occurred; we are not informed. At any rate, we can not suppose his food remained long in the alimentary canal; or that it was digested and absorbed. In this condition, what was there to make bile of? It would require very efficient stimulation of the liver, indeed, to induce an abundant formation of bile independently of its sources. One might about as well deny the anthelmintic properties of a medicine that expelled only half a dozen worms—by the way all there were—when he had set his heart on getting away scores of them.

A considerable portion of Dr. Brown's essay is devoted to showing that in the history of healthy persons exposed for a long time to a moderate mercurial influence—as to exhalation from crude mercury—mention is made of all manner of suffering, disease and death, without a single allusion to an abundant secretion of bile. These facts are doubtless entitled to full credit; but their bearing on the subject is too remote and ill-defined to require discussion here.

Dr. Brown says among many other things, that the bile certainly has some influence on the color of the fæces, or its suppression in jaundice would not produce the change that so commonly attends this affection. But he concludes, from numerous statements, conjectures and opinions of several medical students and others, that *bile is not the coloring ingredient;* and, finally,

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that what is called *bile* is really a secretion from the *intestinal follicles*, and often from those of the colon.

Although it is abundantly apparent mercury is a cholagogue—that it has the power of promoting, especially the depuratory function of the liver—it is equally apparent that functional torpor, and the consequent accumulation of effete matter in the blood, are conditions indispensable to the clear and obvious manifestation of this power. In this respect mercury is like many other medicines—it requires an appropriate disease to illustrate its remedial value. The reputation of opium, as an *anodyne* for instance, would rest on a slender basis indeed, were it administered only where there is perfect freedom from pain. Nay, under such restrictions it would soon acquire the reputation of an efficient *cause* rather than *cure* of pain. But will mercurials succeed in every case where there is functional torpor of the liver?

If we have an obvious want of bile in the stools—an obvious accumulation in the blood of the effete matters from which the liver makes bile—apparent on the tongue, in the conjunctiva, skin and urine, and in its pernicious influence on the powers of life—we have unquestionable indications of torpor of the liver; and, shall we say, an unquestionable indication for the use of mercury? Behind this functional torpor we may have inflammation of the gall duct with temporary or permanent closure; the same duct may be more or less perfectly obstructed by biliary calculi, or by permanent enlargement of adjacent parts; we may have acute yellow atrophy, or the chronic form of this affection, we may have cirrhosis, fatty liver, pigment liver, cancer or tubercle; in all of which cases, if we use mercury, we shall almost certainly do no good and may do much harm. Duodenitis, another very common source of jaundice is, in my judgment, nearly always better treated by leeches and cold applications to the epigastrium, iced lemonade and mild salines than by calomel. For here again the hepatic trouble consists in obstruction of the excretory duct—not in torpid function of the gland.

In functional torpor of the liver, without mechanical or other insuperable obstacle to its operation, calomel usually acts promptly as a cholagogue. When it fails to do this, an obstinate persistance in the use of the article —heedless of the many possible obstacles to its action—some of them exceedingly obscure, and difficult to be recognized at the bedside—and of the fact that this medicine should never be administered with indifference—indicates a low standard of discretion for the Doctor, and a questionable future for the patient.

In our autumnal fevers the function of the liver is mostly obviously impaired. Emetics, cathartics, and even cholagogues were at one time reckoned among the important elements of a successful treatment. Yet I need not say to this audience—what western experience has made abundantly obvious—that in most simple cases of this nature sulphate of quinine alone

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is quite capable of removing the disease, and restoring both secretion and excretion, in a prompt and satisfactory manner. Yet in some aggravated cases, especially when vomiting is a troublesome symptom, a full cathartic of calomel is of great value in promoting both the comfort and cure of the patient. And in an algid state, which we sometimes see in the congestive form of this fever, I have no doubt of the value of a very large dose, sufficient to secure at once the full cholagogue influence of this medicine.

The veins of the portal system being without valves, and consisting of capillaries at both extremities, are capable of ready relief in case of over-distention in two ways,namely: by the progress of their contents forward through the liver in the natural course of the circulation, and by a retrograde movement and the discharge of blood, or its more fluid elements, into the alimentary canal. The excessive discharges, both by vomiting and purging, of cholera infantum, cholera morbus, malignant cholera, and certain forms of diarrhea, are due to this latter mode of action, and are generally destitute of bile. In the early stage of these affections, before irreparable mischief occurs, the cholagogue action of mercury is of inestimable value. It secures a normal, forward movement of the contents of the portal vessels, as well as active depuration by the liver. Owing to the irritability of the stomach and bowels, and the difficulty of securing absorption against so rapid a current of exosmosis, this end is not always easily attained. Very small doses of calomel, frequently repeated, will, however, usually succeed. For instance, in the early stage of acute cholera infantum, a grain of calomel and half a grain of ipecac, rubbed with a scruple of white sugar, and divided into twenty powders—one to be given in half a teaspoonful of cold table-tea, at intervals varying from fifteen minutes to an hour, according to the urgency of the symptoms, and kept up till the characteristic bilious stools appear, withholding drinks—in my hands, long since superseded all other treatment.

While mercury is highly valuable on account of its cholagogue and other medical properties, it is capable of doing irreparable mischief; and its indiscriminate use is in the highest degree reprehensible. A knowledge of its constitutional effects, and the readiness with which they are sometimes induced, should prevent its administration, except in conformity with the clearest indications; and even then the safety of the patient demands the exercise of both vigilance and discretion. It is in this respect, like all other powerful medicinal agents, highly valuable when judiciously employed; otherwise, sure, in the long run, to do much more harm than good.

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THOUGHTS ON ASIATIC CHOLERA,

ITS MODE OF PROPAGATION AND PROPER TREATMENT.

BY L. R. JOHNSON, M. D., CAMBRIDGE CITY, IND.

The State Medical Society, at its last session, did me the honor, (as I am informed, not having been present at the time,) to request of me an essay on a subject of my own selection for its present meeting. Circumstances have prevented me from performing that duty in a manner consonant with my own feelings; but not wishing to make an entire default, I ask the indulgence of the Society for presenting a few thoughts on the disease commonly known as *Asiatic Cholera.* I shall not attempt a thorough discussion of the subject, and shall aim only to submit the results of my own experience on points of practical interest.

During the prevalence of cholera in 1849, at an early period of my professional life, I resided at Columbus, Ohio, and had the good or bad fortune to witness and share in the labors incident to a pretty severe visitation of the disease. I was at the time an undergraduate, practicing under the supervision of my preceptor, Prof. S. M. Smith, of Starling Medical College, who had an extensive cholera practice; and my situation was such as to render me tolerably familiar with the views of the proffession in different portions (especially the northern and southern,) of the State of Ohio, at the beginning and during the progress of the epidemic; and to enable me to compare their opinions in regard to its treatment with the results of my own experience. My subsequent residence in Cincinnati made me acquainted with the history of the disease during the period of its prevalence in that city, where, also, I saw a few cases in 1851; and where it was my especial good fortune

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to enjoy the intimate acquaintance and friendship of Thomas Carroll, M. D., and to find in his observations on Asiatic cholera, as it appeared in Cincinnati in 1849–50, (published in the *Western Lancet* for June, 1854,) a very thorough and able defense of the opinions which, in common with many others, I had been led to entertain. To that paper I am indebted for most of the facts to which I shall here refer, and for much of the confidence which I feel in the views here expressed.

It seems to me a point as clearly proven as almost any other in relation to disease, that cholera is contagious. By this I do not mean to assert that it can be propagated in no other way than by actual contact; nor that every person brought into contact will suffer from the disease; though I suspect that the more closely we investigate, the nearer we shall approach the conclusion, that contagion is the principal if not the sole method of its dissemination. But on this occasion I merely mean to say that, in my opinion, cholera may be communicated from one person to another by means either of direct contact, or through the medium of the excretions. The whole history of its origin and progress on this continent seems to me conclusive on this point. Neither of the great visitations of 1832 or 1849 originated in the interior. On the contrary, its first appearance on both these occasions was on the seaboard, and coincident with the arrival of vessels, from a foreign port where cholera was known to prevail, with cases of the disease on board.

Whatever theory we may adopt, this fact remains: that cholera was unknown, in the south-western portion of the United States at least, until the arrival at New Orleans, in November, 1848, of the emigrant ship Swanton with passengers suffering from that disease. Now one of two things is true. Either it was communicated, by means of a specific poison, from the passengers on board that vessel to persons on shore, or it was produced by some of the general causes upon which epidemics are usually supposed to depend, as, for instance, a particular condition of the atmosphere; in which latter case we have a remarkable coincidence of the atmospheric or other morbific conditions prevailing at New Orleans, immediately after, but independent of, the arrival of the vessel, with those in active operation at the foreign port from which she sailed. Which of these theories is the more plausible, we leave for others to decide. In regard to most maladies, the circumstances would be thought to prove the existence of contagion.

From New Orleans, as from a common centre, cholera was disseminated throughout the west and south-west, along the leading lines of travel; the rapidity of its progress being in exact proportion to the amount and speed of travel. There may have been *apparent* exceptions to this rule, but that it *was* the rule, I think no intelligent observer will deny. Nor is there any more doubt that it was brought to Cincinnati by steamboats on the Ohio river, than that it was brought to New Orleans by the Swanton. Cases

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were carried from the boats to the Commercial Hospital at that port long before its appearance in the city, and at a period of unusually good health. The first cases occurring in the city were among persons who had been brought into contact with patients from the boats. From December 25th till the middle of January the cases were all of that character. At the latter period the Board of Health ceased to report; but a leading physician informs us that though he had at the time an unusual amount of professional business, he did not meet with a case of cholera until early in May, though occasional cases were known to have occurred from the time of its first appearance up to that date. Proceeding thus gradually the whole community was finally involved; the disease, however, seeming to have exhausted itself in some districts when just commencing its work of destruction in others.

Undoubtedly, filth and want of ventilation acted as predisposing and auxiliary causes of the pestilence, though its inroads were by no means confined to filthy localities. Indeed, these causes seemed to have less to do with its progress than the density of the population, and the propensity of the people to assemble in large crowds. The Catholic church, of which this is a charteristic, lost 3,000 of its members in 1849. One congregation alone lost 1,300. On the other hand, the Cincinnati Orphan Asylum, though surrounded by cholera in its greatest intensity, and though its windows were kept constantly open, for the purpose of ventilation, was almost exempt from the disease; only two fatal cases occurring during the summer of 1849. The facts in this case would seem to show that cholera is not the result of any particular condition of the atmosphere; for the condition of the surrounding population, in regard to cleanliness, ventilation, &c., was probably not different from that of previous years. During the summer referred to, in a period of three weeks, seventy children were admitted to the Asylum, from houses or boats where one or both parents were said to have died of cholera. These were thoroughly washed in soap and water before admission among other inmates; and their clothes were thoroughly boiled before being again used. To these precautions the Asylum was probably indebted for its escape from the disease.

Cities, however, are poor places for deciding the question of contagion; and it is to be regretted that the prevalence of the anti-contagious theory among physicians has prevented that thorough investigation in regard to its origin in isolated localities, which the subject deserves. Yet in many of these places, where it has seemed to originate spontaneously, patient inquiry has succeeded in tracing its origin to remote centres of the disease. An occasional failure to do so, however, does not disprove the contagiousness of cholera any more than a similar failure would disprove the contagious character of rubeola or scarlatina. I have more than once met—as I presume most physicians have—with undoubted cases of both these diseases, without

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being able to trace them to a contagious origin; and a few weeks since I had a case of small pox which the most careful inquiry failed to connect with any other case of that disease.

Carroll and Sutton, however, have given numerous instances in which persons removing from an infected to an entirely healthy district have, in a few hours, been attacked by cholera; their attendants and visitors have soon after been attacked, and, in some instances, have died; while *no other persons* in the entire neighborhood, during the whole season, have been in any way affected. In Dr. Sutton's report on cholera, (published in the Transactions of this Society for 1853,) it will be seen that, in a large majority of the counties of this State where the disease prevailed during its last visitation, the testimony of the physicians reporting to Dr. Sutton shows it to have been probably of contagious origin. The testimony, in many of these cases, is entirely conclusive; and the physicians reporting the facts are among the most respected members of the profession in this State.

Watson, in his "Lectures on the Theory and Practice of Physic," informs us that Prof. Simpson, of Edinburg, Scotland, has collected a vast number of examples of such a character as to lead him (Watson) irresistibly to the conclusion that cholera is contagious.

Now, I submit that even one well attested case of this positive character is sufficient to outweigh any amount of mere theorizing; and that the united testimony of so large a number of them, from reliable and intelligent sources, is not neutralized by assertions of the non-contagious character of cholera, based upon a failure of such observations by others. And here I leave the subject.

The idea that cholera is in any way dependent upon the use of lime water, is effectually disposed of by the single fact that on Rittenhouse street, Cincinnati, where the heaviest mortality prevailed, there was but a single well, and to this but a few persons resorted, only one of whom died of the disease; while all others on that street used rain water exclusively.

Almost equally certain is it that the condition of the atmosphere, as to temperature, moisture, or electrical changes, had as little to do with the prevalence or severity of cholera. Such, at least, was the opinion of Prof. Joseph Ray, himself a physician and a careful and accurate observer; with his own meteorological tables for many preceding years, and the daily cholera reports before him; and the facts which he adduces are strongly confirmatory of that conclusion.

While on this subject, however, I may mention a circumstance coming under my notice at Columbus, Ohio, in 1849. Near the close of the epidemic, a prominent gentleman of that city remarked to me, that for the ten preceding days a comparison of his dry and wet bulb thermometers had shown a perfect saturation of the air with moisture; but that, on that morn-

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ing, he had noticed a difference of ten degrees, (showing a diminution of the moisture,) from which he predicted a decrease of Cholera. The event justified his expectation, but whether as a result or a coincidence I am unable to say.

The symptoms of cholera are so well known to most physicians that I need not detail them here. I, therefore, proceed to the treatment of the disease, in regard to which as great a difference exists among members of the profession as upon the question of contagion. In this, however, it is consoling to know that our ignorance is supplemented by the wisdom of the people. The religious and secular press know "all about it." A missionary on a foreign station, (who has seen the vast number of one hundred cases,) informs the world that three-fourths of all persons afflicted with cholera are drunkards; and straightway proceeds to announce, *ex cathedra*, the treatment of the disease. His views are deemed of sufficient importance to receive the commendations of no less a person than the "Commissioner of Statistics" of the State of Ohio, who thereupon advises "every family in the land to procure a copy of the missionary's letter, to be kept constantly by them for reference." The letter is issued in pamphlet form by the publishers of a leading daily journal, who thereby "turn an honest penny;" is purchased in large quantities; and the people, rejoicing in the opportunity thus afforded, of becoming as learned in the treatment of cholera as all the doctors, are induced to rely upon a mode of treatment which, in serious cases of the disease, will infallibly kill more than it will cure. But then they have the missionary's word for it, and they are sure he must know, especially as gentlemen of the cloth are known never to encourage quackery!

Among physicians practically acquainted with cholera with whom I have conversed, there has been a very general concurrence of opinion in regard to its treatment. At the outset the opinion prevailed that, being a new disease, it would have to be combated by new remedies or new combinations; while by those who adhered to the old remedial agents it was generally conceded that powerful doses of those articles would have to be employed. I think there were few physicians who did not, in the beginning, consider large doses of capsicum and other heating drugs an indispensable part of the treatment. Camphor, also, in large quantities, enjoyed the confidence alike of the profession and the public. With observant men whose views I had an opportunity of learning, actual experience soon corrected all this. I well remember that, early in the epidemic of 1849, prominent physicians of Cincinnati advised the use of Cartwright's prescription, viz.: Calomel, grains, 20 to 60; camphor, grains, 15 to 30; capsicum, grains, 20 to 40—mix; the whole to be taken at one dose, and repeated in from two to four hours, alternating with combinations of tinctures of capsicum, camphor, opium, sulph. ether and various essential oils. In some cases, also, if I remember rightly,

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ten or twenty grains each of sulphur and charcoal were added to the above. At the same time the Cleveland Board of Health, acting under the advice of their leading physicians, published, *pro bono publico*, the pamphlet of Hawthorne, of Belfast, Ireland, recommending the repeated use of 10 grains powdered opium, 10 grains, or more, of camphor, with chloric ether and other powerful agents now forgotten. Nor shall I ever forget with what unanimity the physicians from both these places, at the meeting of the State Medical Society at Columbus, near the close of the epidemic, reprobated the use of large doses of any remedy in cholera, and united in recommending something like the treatment adopted by Ayre, of Hull, England, (viz.: one grain of calomel and one drop of laudanum every five minutes,) which some of us at Columbus had found to be the most effective mode of treatment.

In my experience the use of all these heating drugs has been "evil and only evil, and that continually;" producing increased irritability of the stomach, or, if retained, inflammation of that organ, or subsequent dysentery; while the legitimate effect of such large doses of opium is to check the action of the liver and induce coma.

Whatever the pathology of cholera may be, there is constantly present a suppression of the biliary secretion; on the restoration of which, in ninety-nine hundredths of the cases, the patient recovers. To effect this object, I know of no remedy so reliable as calomel. Opium is of service by assisting the stomach to retain it till its effect upon the liver is secured; and brandy does good, (if at all,) by sustaining the system until that effect can be obtained. They are adjuvants to the calomel which cures the disease; if, in these progressive days, we may be permitted to suggest that medication does have any known effect upon the human system. These remedies, with counter-irritation to the abdomen and, perhaps, the spine, by mustard poultices or turpentine epithegms, warmth when needed to the extremities, friction to relieve cramps, a rigid enforcement of the recumbent position, and total abstinence from the ingestion of every other article, solid or liquid, constitute, in my opinion, the whole rational treatment of a case of cholera. Even the calomel and opium, if administered in large doses, are likely to do harm. In smaller portions they are more likely to be retained, and to act upon the liver rather than the bowels. At the outset of the disease, if the stomach will retain five grains of calomel and half a grain of opium, they should be given, and repeated every ten, fifteen or twenty minutes, until three or four portions have been taken; after which the quantity, especially of the opium, should be diminished. In many cases it will be found necessary to commence with the smaller doses at once; and by giving a grain of calomel and a drop of laudanum, or its equivalent of opium, every five minutes, it will be found that a larger quantity will have been retained at the end of an hour than where larger portions have been given. Indeed, it is

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my experience that the attempt to administer even the smallest of Cartwright's doses above referred to will, if persisted in, prove fatal to the patient; and I speak after repeated and earnest efforts to induce their retention. How others can succeed with them is beyond my comprehension.

The calomel should be continued, through all stages of the disease, until the biliary secretion has been fairly established, as evinced by the appearance of bile in the stools, or the disappearance of all the cholera symptoms. There is no case so desperate as to justify its withdrawal; for under its administration patients who have continued sixty, and even seventy-two, hours in profound collapse have recovered. Dr. Carroll had twenty-nine cases in which there was an entire absence of pulse at the wrist or temples, continuing for various periods of from *four* to *seventy-two* hours; yet of these nineteen were carried successfully through this stage of collapse by the administration of two or three grains of calomel with a twentieth to a sixtieth part of a grain of opium, and a teaspoonful of brandy, every ten minutes. When given in doses of one grain every five minutes, I have known 200, and even 300, grains of calomel to be taken, when the patient would recover without ptyalism. The opium should not be continued after three or four grains have been exhibited, for the reason before assigned. If continued at all during collapse, it should be in minute doses, with a view to its stimulant effect. In this I am sustained by Ayre, Carroll, and others of equal authority.

Whether any other stimulant than opium can be profitably employed in the treatment of cholera, is a question about which the advocates of the above mode of treatment entertain different opinions. Carroll uses a teaspoonful of the best brandy after every dose of calomel, and aims to give about an ounce every hour. Ayre, on the other hand, strongly deprecates the use of alcoholic or other stimulants in any quantity. It is with extreme diffidence that I venture to dissent from the views of so able and successful a practitioner as Dr. Carroll, whose success in the treatment of collapsed cases of cholera was greater than that of any physician in the West, or, (as I believe,) in the United States. But even his success, as he frankly admits, was not so great as that of Ayre; and I observe, that of his nineteen cases carried through collapse seven died of consecutive fever. Now, Ayre claims, as a proof of the superiority of his mode of treatment, that there is no consecutive fever following. My own experience is small in comparison with that of Dr. Carroll; but in the cases which I saw, either alone or in connection with my preceptor, little or no alcoholic stimulus was used, and secondary fever was almost, if not quite, unknown. The patient once relieved of active cholera symptoms, we felt little anxiety about him, excepting that arising from the dread of a relapse.

Upon another point in the treatment, I have no hesitation in endorsing Dr.

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Carroll's opinion. In cases of collapse, where the system has been thoroughly drained of its fluids, he insists that the amount of blood upon which the heart contracts is barely sufficient to stimulate it to action; and that if this be diverted to the surface or extremities by the application of heat, the patient will die. Consequently he uses only gentle friction to relieve cramps, excluding hot applications externally for a period of six or eight hours, in cases of collapse. Of the correctness of this practice, and the reasoning upon which it is based, I have no doubt; and the same objection seems to me to apply, to some extent, to the use of stimulants in any considerable quantity, under the same circumstances.

I should have said that a plentiful supply of fresh air is a most important part of the treatment in every stage of the disease. When reaction has been fairly established, it is a matter of prime importance that the patient shall not abandon his recumbent position, which nothing short of physical force will, in many instances, prevent him from doing. In more than one instance, when all the symptoms of the disease were apparently removed, I have known relapse and death to result from merely rising up in bed. This should never be ventured upon until the recurrence of bilious evacuations from the bowels, and the thorough quieting, for some hours subsequently, of any attendant irritation. I have, also, known death to result, after apparent recovery, from swallowing a few spoonfuls of ice cream.

Another indiscretion will have to be avoided if the life of the patient is to be preserved. In cholera, as in dysentery and typhoid fever, there is an almost insane propensity on the part of the patient and his friends to administer "a dose of physic" if the bowels have not been moved for a day or two. Especially will this be the case if the individual supposes himself to have taken calomel. Yet nothing can be more dangerous. Bilious evacuations, without excessive purging, are, doubtless, to be desired as affording the crowning proof that the disease has been, for the time at least, subdued; but a very slight indiscretion in this direction will blast the hopes alike of physician and patient. For two or three days nothing more active than an injection of warm water should be used. Should bilious diarrhœa supervene, I would depend upon mucilaginous drinks, counter-irritation and small portions of opium. Should suppression of urine continue after the disappearance of other symptoms, the ordinary diuretics, (sweet spirits nitre as good as any,) should be resorted to.

Under the kind of treatment here advocated, cholera, when early subjected to treatment, has seemed to me as amenable to treatment as any other disease. Cases will occasionally die, however, in spite of all our remedies, even when given at the very outset; but this is not the rule. When threatened with cholera the patient should immediately lie down and remain quiet, applying a mustard draught over the whole abdomen and, if a physician should

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not be at hand, swallowing a teaspoonful of paregoric, and applying warm applications to the feet if they are cold. But no time should be lost in procuring a physician, whose instructions should be obeyed to the letter. In no other way can a recovery be expected.

In regard to the prevention of the disease, I have time and space to say but little. Whatever we may think on the subject of contagion, the facts that cholera is brought to this country on shipboard, and that, for weeks past, it has been confined to the limits of a quarantine station, are, to my mind, clear indications of the propriety of enforcing non-intercourse with cases of the disease arriving from abroad. Should this measure fail to prevent its appearance among us, the principal precautions to be observed are, cleanliness, free ventilation, the avoidance of crowded assemblies, the use of a plain but substantial diet, excluding crude vegetables, and vegetables containing only a small amount of nutriment; and, in general, the observance of such a regimen as has been found most conducive to the mental and bodily vigor of the individual. Intemperance is, undoubtedly, a fruitful source of the disease; but the period of a cholera visitation is not the most favorable time for a drunkard to become a "teetotaller," though it is the time for him to abandon his drunkenness. Sudden changes in the mode of living, if that has been reasonably prudent, should be avoided.

The effect of fear in the dissemination of cholera has been, in my opinion, greatly exaggerated; but, beyond question, it was a potent ally of the cholera poison. This it is the physician's duty to allay to the extent of his ability; and it can only be done by the inculcation of correct views in regard to the disease, and the methods of its propagation. Men will resolutely face any danger, the extent of which they can accurately measure, while they will flee precipitately from a hidden foe whose capabilities for mischief they can not estimate. A general belief that cholera is contagious does far less mischief than the prevailing ignorance of the means of prevention; and a failure to instruct the public on these points is the legitimate result of disbelief, on the part of physicians, in its communicability. In this, as in everything else, the people should be told, and have a right to know, the exact truth; and when they learn that, though cholera is contagious, yet many persons exposed to its influence do not contract the disease, and that the chances of escape will be greatly increased by cleanliness, ventilation, the instant removal and burial of cholera discharges, &c., the terror which its presence inspires will be greatly diminished, at least among intelligent people. In this way, too, the confidence of the public, in the candor and intelligence of the profession, will be retained; while it will be shaken, if not destroyed, by the denial of its contagious character in the face of numerous facts by which the people generally are confirmed in a contrary belief.

A striking illustration of the sanative influence of free ventilation is found

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in the fact that in the entire row of houses opening on the Ohio river, at Cincinnati, extending from Mill creek to Fulton, (a distance of three miles,) and including a large number of the lowest drinking holes of the city, there were, in 1849, very few cases of cholera; and most of those that did occur were cases brought from boats on the river. In the drinking holes above referred to, it is said that only one or two fatal cases occured in either year, though the whole number of deaths in the city in 1849 was little, if any, short of 6,000. This remarkable exemption was probably owing to the fact that currents of fresh air are almost constant along the river, and that these dens of iniquity were closed only a few hours of the day or night.

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CHOLERA.

BY WM. F. HARVEY, M. D., PLAINFIELD, IND.

I shall only consider the cause and some of the more prominent symptoms of cholera, for the purpose of indicating the proper remedies.

The first and most frequent symptom is either a gentle diarrhœa, or a feeling of depression, soon followed by diarrhœa, which continues until the stercoraceous contents of the alimentary canal have all passed away; and is followed by serous discharges, with or without pain, cramping and vomiting. Soon after the serous discharges commence thirst begins; yet, drinking water is followed by vomiting and cramping in the extremities. These symptoms continue until the collapsed stage is reached, in which the serous portion of the blood is wanting in a very great degree, and the remaining carbonaceous portion is thick and tenacious, and will not pass through the capillaries; hence congestion occurs in the whole body, with a blueish and shriveled condition of all the soft parts, followed by cold extremities and death. Where there is neither pain, vomiting nor purging there is frequently great depression of the nervous energy.

There are three causes—a primary, a secondary and a tertiary—which, in the order named, are as cause and effect, in a great measure, to each other. The primary cause is electrical, and being general, man has no control over it. It is a disturbance of the equilibrium or distribution of electricity. This has been my opinion ever since I saw cholera cases in 1849, in Kansas, and observed the meteorological condition of the atmosphere, and the workings of an electrical machine with which I was occasionally experimenting. Pre-

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vious to the appearance of cholera in the neighborhood the machine worked well, and I could collect the electric fluid in a leyden-jar without any trouble; but after the disease appeared I could not get a spark, and this continued until the subsidence of the epidemic, when I could again collect electricity. During the prevalence of the disease there was no thunder or lightning, and but little rain. For a week, during the time the disease was the most extreme and fatal, the thermometer indicated 110° F., in the shade, a few hours each day.

During the time the cholera raged the air was filled with watery vapor; and I concluded that the electricity was distributed through this agency. In its passage through the air, in this manner, the secondary cause, nitrous acid, NO4, or nitric acid, NO5, is produced by the union of the nitrogen and oxygen of the air. These acids are generated by the passage of currents of electricity through nitrogen and oxygen in a close vessel, and why may not the same result take place in the open air? The fact that nitric acid has been discovered in rain water after a thunder storm goes to prove that it does so occur. Nitrous acid is volatile above 82° F., and although decomposed by water into nitric acid and deutoxide of nitrogen, there is a strong probability that the watery vapor may be saturated with nitric acid, and the heat of the sun cause the more volatile nitrous acid to be disseminated through the air. That such is the fact I have no doubt. The whole atmosphere, in certain localities, being charged with these acids, they enter the system during respiration and by ingestion, and decompose the blood.

During the prevalence of cholera, almost every one feels an uncomfortable and indescribable sensation. The peculiar feeling of depression, to be understood, must be felt. This sensation is caused by the action of the poison primarily upon the blood, and secondarily upon the nervous system. In many persons nature prevents the poison from overwhelming the vital powers until it can be expelled. Persons with weak nervous systems are unable to prevent the poison from producing its effect in the blood; hence they have cholera.

Suppose nitrous or nitric acid taken into the system, how can they produce cholera? I answer, by acting upon the blood, decreasing the crassamentum and increasing the serous portion. The increase of the serous portion of the blood excites the surfaces of the alimentary canal to discharge it from the system. So much of this constituent is discharged from the blood, that what remains is not sufficient to carry the fibrin, albumen, &c., through the capillary vessels; hence congestion follows, the skin and tissues become shriveled, and fatal consequences soon follow unless averted by remedial agents.

Fear produces more cases of the disease than all other exciting causes. The poison depresses vital energy; but fear crushes it to such an extent that

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if no other cause were operating in the system, many persons would be attacked by a disease closely allied to cholera.

Does contagion or infection have anything to do with the spread of cholera? I think not. The disease is atmospherical wholly, and when one person is liable to it, all persons in the same region are liable to it; and those who regard it in this light are much less liable to contract it. It does not travel invariably with the travel of the world, but makes its appearance remotely from the great thoroughfares, simultaneously with its appearance in localities along the route of travel. There is disturbance of the electrical currents wherever cholera exists. Electricity passes regardless of winds; so does cholera. Hence I conclude that the disease is neither contagious nor infectious, but epidemic; and there is no more reason for running from it than from a bad cold. Those who are with the disease most, and fear it least, fare the best.

The indications for treatment are, first, to neutralize the poison; second, to sustain vital energy.

There is no agent which fills the first indication equally to calomel. It enters the system as chlorine and mercury; the latter unites with the acid forming a neutral nitrate of the suboxide of mercury, (or a nitrite); and chlorine is liberated, and acts as a restorative, destroying the putrefactive tendency, and enabling the vital powers of the system to rally again. The following, Cartwright's, prescription is the best I have ever used:

Submurias Hydrarg, grains, XX to LX.
Pulv. Camphor, grains, XV to XXX.
Pulv. Capsic. Ann., grains, XX to XL.

Mix, and make one powder, to be given and repeated every two to four hours, until vomiting and purging cease. If vomiting occurs immediately after a dose is taken, repeat the dose at once. Give also from a teaspoonful to half of a tablespoonful of the following mixture after each evacuation from the bowels, or act of vomiting:

Sulph. Ether.
Tinct. Capsic. et Myrrh.
Tinct. Camphor. aa. f℥i.
Tinct. Opii.
Ess. Menth. Pip. aa. f℥ss.
Ess. Cinnamon. f℥i.
Ol. Caryophyll. f℥i. Mix.

When there is extreme spasmodic pain, I would add to the above chloroform f℥ii. Stop the vomiting and purging instantly if possible, and the pain and other symptoms may be more easily controlled.

In addition to the above, use friction, and apply powdered capsicum to the extremities; and apply a blanket or other large flannel cloth, wet with water

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as hot as can possibly be borne, over the entire abdomen, and keep the cloth hot. Give no water to drink, no matter how urgent the thirst, except enough to administer the medicine, until several hours after vomiting and purging have ceased. If you permit your patient to have water, medicine will do no good.

Whisky and such stimulants are dangerous. Persons habituated to their use, have never recovered from an attack of this disease, as far as I have observed; and I have known many such to die.

The best prophylactic is a large dose of calomel and capsicum. It should be taken when the peculiar depressing feelings come on. I would use sulphate of quinia in doses of from ten to twenty grains, during the stage of collapse. It is an indispensable remedy, with those mentioned, during the congestive stage.

Ice to the spine, and soda internally given, have been recommended, but I would not use them. I would apply a mustard poultice along the whole length of the spine rather than ice.

I have invariably found opium injurious when given in powder, or in full dose in any other form. It prevents the action of calomel by its narcotic property.

These views of cholera and its treatment were formed while observing the disease during four partial epidemics, occurring in the years 1849–52–54 and '55.

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THE PATHOLOGY AND TREATMENT OF CHOLERA.

BY R. E. HAUGHTON, M. D., RICHMOND, IND.

In looking over the literature and history of this very grave and dangerous epidemic, we find much that is strange and fanciful, yet much to invite our attention in its investigation. As yet, though now threatened with a third invasion of it in thirty-three years, but little is known of its cause, modes of travel, pathology or successful treatment. Yet this should not prevent us from attempting whatever may be done in these particulars, in view of its great mortality. I propose to present, in the light of such principles as I think are sufficiently well established in pathology, physiology, &c., a rational pathologic history; and erect thereupon a basis of successful practice. I find medical men everywhere conceding that they do not understand its nature; it is, therefore, proper, in view of a coming invasion, to investigate it with all the knowledge we have, and with such zeal as may throw light upon what is admitted to be an obscure disease. We find that it progresses along the world's great highways, and visits its centres of trade and commerce; and that heretofore all sanitary regulations have done little more than simply to check its progress for a short period of time. In examining the phenomena which are presented in this disease, we must investigate in several directions, with a view to interpret for ourselves what is observed, and thus be able to give expression to laws which will be the written history of its various stages.

It was a cardinal doctrine of Broussais that irritation, or an exaltation of vital force, is at the foundation of every morbid condition, and that such irri-

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tation always results in an increased flow of blood to the part. The latter part of the proposition, if not all of it, in the progress of anatomical research, has been proved not to be true; but that some irritation operates to produce disease, is a fact which finds expression in the doctrines of cell pathology. While we are investigating tissues, we must consider,

1st. Vital action, and upon what it is dependent.

2d. The blood, and its relations to tissues.

"Every vital act," says Virchow, "presupposes an excitation, or, if you like, an irritation." Now, in disease, we must deal with both function and nutrition. Every act of an organ in its healthy condition is an expression of its function, which implies its healthy nutrition; and every departure of the organ from such a standard is the expression of disease, which involves its nutrition. Tissues are made of the ultimate molecules or cells; and the vital power, which presides in all the tissues, is suspended or destroyed by any irritation which disturbs the harmony of cell or molecular action. The vital power of selection, or chemical affinity, is destroyed, and we have a mal-nutrition, or no nutrition, as a result. The original irritant may be one acting upon the part where the disease is expressed; or it may be the expression of some morbific cause acting primarily upon the nervous system, and reflected through the nerves to certain tissues.

In cholera the functions of all the organs of secretion are arrested. Dr. McLeod, who saw much of cholera during twenty years in India, says, "Were I asked to state, as succinctly as possible, in what cholera seems to consist, I should say total suppression of some secretions and great excess of others." One of the most prominent symptoms of this disease is profuse and exhausting discharges from the alimentary canal. What causes such a quantity of fluid to pass so rapidly that exhaustion is soon the result? The secretions of the liver, kidneys, salivary glands, and most probably of all the glands, are arrested; and there is a want of that harmony which is expressed in the healthy action of all the organs; and when this harmony is destroyed, the function being arrested, vital affinity, the power of cell selection and elaboration, destroyed by the impairment of the vital force residing in the nerve centres, a diseased or poisoned condition of the blood results from a failure of the organs to eliminate this poison. The effete elements being retained, and there being no outlets save through the bowels and skin, there is established the vicarious action of these two extensive surfaces. Hence arise the copious perspiration and watery discharges from the bowels, producing complete exhaustion, to eliminate these deadly retained elements.

Each cell has its origin in the existence and duration of a formative or life force, which must have been impressed upon all organisms, and derived from a parent cell. The duration and activity of any cell is the expression of its vital force; and the force resident in each cell has a limited and definite du-

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ration; and the production and elaboration of new cells evidences a continuation of the life force until some agency, which has a power to degrade it, unfits it for healthy nutrition. Under such degradation of force we have, not only cholera, but all other diseases of an exhausting character, and zymotic or blood diseases. I do not regard this life force as having its origin in the cells, or in their activity, but in the nerve force which presides over all the functions of organic action.

In cholera we find a deficiency produced in the normal condition of the blood by the rapid abstraction of its watery elements; and, therefore, as the secretions are all produced from the blood, when the blood is deficient in the elements necessary to secretion, it can not take place. Secretion in cholera is arrested in all the glandular organs.

In observing the vital manifestations in an inverse order, beginning with nutrition and going back to the first link in the chain of morbid events, through the processes of secretion, excretion, circulation and innervation, we find that some impairment of the life force has been produced, and we see all these processes arrested; which, taken in the aggregate, we call disease. These processes are not impaired or arrested without a special influence on the blood; and the blood can not furnish the materials of nutrition. Nerve force requires blood which is complete in its normal elements to produce its perfect manifestation; hence the relations of blood to nerve tissue and nerve force. Healthy nerve tissue is necessary to the production of healthy blood and all tissues; hence we say that the first manifest impression, as evidenced by the prominent symptoms, not only in cholera, but in all idiopathic diseases, is upon the nervous system. Lesion of innervation is the first link in the chain of morbid impressions; lesion of circulation comes next; then lesion of secretion and excretion; and, finally, lesion of nutrition. Dr. Southwood Smith teaches this doctrine in the pathology of fevers, and although not among the more recent writers on morbid action, I have learned, in the field of pathologic investigation, from his pen, that which no new investigations have as yet disproved. He says: "We discover that the sympathetic nerve causes the contraction and permits the dilatation of bloodvessels; and the establishment of the doctrine that the main force effecting the circulation of the blood is a chemical one, inaugurated a new era in physiology, and gives promise of one in the field of therapeutics." Dr. Brown-Sequard says, "I consider that the knowledge of the effects of the paralysis and the irritation of the sympathetic nerve opens a new field in physiology, pathology and therapeutics." Prof. Laycock says: "Among the morbid phenomena dependent upon derangement of the nervous system, which regulates their production, are purpura hemorrhagica; acute and chronic fluxes of serous fluid from free surfaces; effusive inflammation; congestion and dropsies." "The recent experiments of Matteucci give additional proof of the analogy

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of nerve force and electricity, and we have reason to believe that the ordinary phenomena of life are dependent upon electricity, acting upon the organism from without."—*Lon. Lan.*, Jan., 1866. Carpenter, in his physiology, says: "It can scarcely be doubted, however, from what is known, that it stands in very close relation to vital force, and is capable of exerting an extremely powerful influence upon its operations." "It seems, then, to be a legitimate expression of the dynamical conditions requisite for the production of the phenomena which we distinguish as vital, to say that they are dependent, directly or indirectly, upon the physical forces pervading the universe, (of which man is an epitome,) which, acting through organized structure as their material substratum, manifest themselves as vital force; one of the most characteristic operations of which is the production of new tissue, which, in its turn, may and does become the instrument of similar metamorphoses. And we have the same kind of evidence that light and heat, acting upon the organic germ, become transformed into vital force, as of the conversion of heat into electricity, by acting on a combination of metals, and heat and electricity into motion, when their self-repulsive action separates the particles of matter from each other. For as we now find that heat, light, magnetism, electricity and chemical affinity are transformable into vital force in organisms, so is vital force capable of manifesting itself in heat, electricity, magnetism, chemical affinity, or mechanical or muscular motion; thus completing the proof of that mutual relationship or correlation which has been shown to exist among physical and chemical forces themselves." (Grove; Corr. of Forces.) "The formative force of organisms," according to Kirke and Paget, "as presented in all the vital acts of nutrition, secretion, absorption, &c., are the results of those various agencies called physical, operating under a controlling and directing force, which we denominate vital or formative force. Hence, in the exercise of the simplest expression of that formative force resident in all organisms which produces a cell, there is evidence of the operation of many forces. Mechanical force is shown in the determination of the position, shape and relations of the cell; chemical force or affinity in the determination of the composition of its walls and contents; and directing and controlling these is vital force, by virtue of which the cell is made to perform its office in the construction of tissue or the elaboration of secretions."

For the proper performance of all vital acts water is necessary; also, for all the changes which the nutrient materials undergo, and for the very structure of a cell itself. Oxygen is necessary for all or the greater number of the chemical actions of a cell. Heat is also indispensable in the proper performance of nutrition; and the vital action may be regarded as the equivalent of the heat developed.

In a severe attack of cholera there is a great diminution of the animal heat; and vital action is destroyed or impaired in proportion as animal heat

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is reduced. The decline of animal heat is owing to the imperfect respiration, causing diminished oxygenation of the blood. The elements present in the blood, from disintegration of the tissues, are not thrown off in the excretory processes, nor can they be in the respiratory process; hence we have blood poisoning, in which the vital force, which produces healthy blood, is destroyed, or so impaired that blood is not formed, and the watery elements are strained through the walls of the vessels. A profound blood poisoning exists, which nature seems to attempt to cure by elimination from the surfaces capable of it. I am more disposed to regard the discharges as an evidence of the destruction of all the vital forces than as an excretion. This seems to me to have a striking analogy to the condition which blood presents after standing some hours; the coagulum being separate from the serum. The vital force, in this disease, no longer acting, the blood is affected by a change of temperature, and the fibrin coagulates. To the process of elimination, or exosmosis, which is rapidly effected, we find no compensating endosmosis, and the elements which make the blood a vital and vitalizing fluid are rapidly carried out. Dalton, in his Physiology, makes this statement as to the blood before and after coagulation, (I quote it to show the analogy to cholera cases in which the blood is separated into its elements): "Before coagulation the blood consists of,

1st. Globules.

2d. Plasma. { Fibrin. Albumen. Water. Salts. }

After coagulation it is separated into,

1st. Clot. { Fibrin. Globules. }

2d. Serum. { Albumen. Water. Salts. }

In cholera the solid materials of the blood are retained as fibrin and globules. Albumen escapes from the blood only in union with water, and is, therefore, the most important element of the plasma of the blood. After coagulation the serum, which contains albumen, water and salts, separates. When strained out by exosmosis, in cholera, we have the separation of the elements of the blood as certainly effected as is done when it coagulates out of the body. No vital act occurs in this condition of the organism. The solid constituents of the blood, which remain in cholera, are perhaps not coagulated, but evidently become thickened, (says Dr. McLeod,) and unfit long to sustain life.

That no absorption takes place in well marked cholera, is proven by the fact that the process of exosmosis is so rapid that the endosmotic absorptive act of the cell can not be performed. It can not appropriate enough of

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reparative material to enable it to sustain the nutrition of the part. From this failure the relation of endosmosis and exosmosis goes on to the destruction of the organism. Hence arises the difficulty in getting the action of remedial agents. The absorbents are not able to act, and, in addition to this, the remedies are often rejected by vomiting.

I believe there is a poison which operates by direct nervous depression, or by absorption through the pulmonary tissue; and, reaching the blood, unfits it for the healthy nutrition of the tissues. The proofs of the action of such poisons are to be obtained mainly from the natural history of such diseases. So far neither chemistry nor the microscope has given us any light upon the essential nature of these poisons. Dr. Carpenter says: "It is the human body which forms the appropriate testing apparatus for poisons." The exhalations of the dead house, the dissecting room, the slaughter-pens and houses of our great cities, give evidence of undetermined poisons; and the injection of putrid matters into the blood, and the use of impure or damaged food give rise to symptoms resembling those of cholera. It seems to me that we have evidence of a blood poison, in the history of the mode of invasion, which is characterized by general malaise, discomfort and derangement of function, for a longer or shorter period, when we have more marked symptoms, to which often fear is added, ending in collapse.

TREATMENT.

If we read carefully the history and literature of cholera, we are struck with the vast dissimilarity of opinion in regard not only to its pathology, but also to its treatment. One thing is absolutely certain, that a disease presenting so nearly a uniformity of symptoms can be successfully treated under all the various notions of its pathology.

That some poison finds entry, in this disease, to the blood through the lungs or gastro-intestinal canal is a point now generally admitted. Knowing, therefore, what we do in regard to the management of many diseases caused by blood poisons, we are not left long in doubt as to what is necessary to assist nature's efforts, which I regard as curative.

We see, first, languor, discomfort and weariness; a little later a sensation of sinking about the stomach, slight diarrhea and loss of appetite; still later we have exaggeration of all the symptoms, nausea, vomiting, purging, coldness and lividity of the skin, great impairment of the volume and force of the pulse, shrinking of the features, sometimes great difficulty of breathing, coldness of breath and tongue, burning heat at the stomach, great and consuming thirst, nearly or complete suppression of bile and urine, copious rice-water discharges, clearness of intellect, sometimes drowsiness and cramping of the muscles; all of which are evidence of the action of some agent which over-

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whelms the organic functions. There is a condition analogous to a violent chill, with severe internal congestions, followed by reaction. The proper treatment in such a case would be heat and cold, consecutively applied, to equalize the circulation and restore the harmony of interrupted function. The same is required in the cold stage of cholera and the reaction. To secure the latter is the primary indication; and to do this we must make an impression upon the ganglionic and cerebro-spinal nerves, thereby stimulating them to send the blood into the capillaries, and restore the vital processes. Heat alone will accomplish these changes, but should be used with caution. Heat is a stimulant, and its absence, cold, a sedative, to the nervous system, particularly to the sympathetic nerves. The latter preside over the vasomotor phenomena, regulating dilation and contraction of the vessels; and as they are stimulated by heat, and reaction goes on, cell transformations go on more rapidly, the blood flows to them more copiously, and greater heat is evolved than in health, and requires control. The effect of heat applied to the spine, or on each side of it, is to raise the temperature of the sympathetic ganglia, which quickens their functional activity.

The first effect of the poison, in this disease, is on the circulation, producing depression or exhaustion, with great diminution of the volume and force of the pulse. The heart is enfeebled, evidencing that some depressing cause is acting on the ganglionic and cerebro-spinal nervous systems, and through them the lungs and organs of secretion and excretion, resulting in congestion of the lungs, liver and kidneys. Uræmia and albumenuria are results of the latter congestion. During the stage of collapse the passage of blood through the lungs is impeded. If a poison is introduced into the blood, besides the deficiency of the heart's action, there is a change of its elements—it becomes thickened, and, on post mortem examination, the right side of the heart and pulmonary arteries are filled with blood, while the left cavities are empty, or nearly so, and the ventrieles firmly contracted. If venous blood is arrested before reaching the capillaries of the lungs it is not oxygenated, nor passed on to the left cavities; hence these are empty—the arterial stream is reduced to a minimum, or arrested entirely in collapse. Magendie "cut across the temporal artery of a patient in collapse and no blood followed." The temporal and brachial arteries have both been opened but no blood flowed. In this condition of poisoning, elimination is the true remedial process; and elimination can best be effected, and these morbid actions relieved, by stimulation. Of all stimulants heat is the most powerful, accomplishing, in my hands in this disease, all the effects attributed by writers to stimulants; relieving pain; removing muscular spasm, tremor or convulsion; relieving or aiding the circulation, and reducing its undue frequency; and reducing excessive secretion. Bloodletting, astringents, opium, &c., are not calculated to inspire confidence in our art. To aid and support nature by stimulation to

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effect elimination of the poison, and sustain the system by nutritives, heat, and the avoidance of all exhausting efforts, with hygienic means, are the indications.

There are three, sometimes four, stages—

1st. Initial.
2d. Diarrheal.
3d. Collapse.
4th. Reaction.

The first two stages are curable, and under efficient management the third also. Nature teaches the mode—by elimination. Our duty is to assist or control her action within proper limits, to get rid of the poison. The rice-water discharges are the serum of the blood, which escapes by exosmosis, with epithelium from the mucous membrane thrown off in the process of elimination. If this process does not exhaust the patient too rapidly the poison will be expelled. Control this process, and sustain the system by internal and external stimulation. When the nausea and vomiting prevent the administration of stimulants internally, resort to external stimulants. In the earlier stages, removal from filthy and imperfectly ventilated localities should be secured. If diarrhea exists, anodynes in stimulant doses, with camphor and capsicum, are indicated, with quietness of body and composure of mind. Nutritious diet and good ventilation will perfect the cure. Alteratives, to act on the glandular system, are beneficial, and prevent collapse. The testimony is almost universal that when there is an increase of the functional activity of the liver and other organs of secretion, there is an abatement of the disease. I do not regard opium as a proper remedy in any stage, except the first, and then only in stimulant doses, combined with alterative doses of calomel. Large doses interfere with oxygenation of the blood, and the elimination of the poison by the excretory organs. If the elimination by the alimentary canal goes on too rapidly or excessively, control it; but if the strength does not fail, aid it by purgatives, emetics, venesection, &c. Before abandoning the use of stimulants, try alcoholic liquors, capsicum, carb. ammonia, and especially, when others fail, tinct. cantharides, in large doses. The last is one of our most reliable stimulants. (I have given teaspoonful doses, when the ordinary remedies failed, in diarrhea and cholera morbus with success.) At the same time use external heat, dry or moist, by hot bottles, hot bricks or bags of hot sand. I have seen the exhausting discharges arrested promptly, and cramp relieved, by its application when no medicine had been given which could have produced the result. Allow plenty of cold water, or even ice, to cool the burning at the epigastrium and supply the loss of fluids. In cases where there are few or none of the peculiar discharges, and fluids are retained in the bowels, such a purgative is indicated.

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as will completely evacuate the bowels without irritating them. Castor oil seems to be the best.

In a paper by Mr. Walkins, (Brit. Med. Jour.) an account is given of the treatment of four classes of cases of cholera. In one class opium was given alone; all died: in the second opium was combined with calomel, or hyd. c. creta; more than one-half died: in the third, of five cases, calomel alone was given; one died: in the fourth, of twenty cases, castor oil was given; two died. The injection of salines into the blood, and hot fluids into the bowels, have been commended by Dr. Resdon Bennett and Dr. Bullar. Friction to the surface of the body, with alcohol, mustard or any fluid remedy, is to be avoided as increasing the tendency to cramp and rendering the surface colder. Local inflammation may be the result of reaction, to control which requires extreme care; it is not evidence of exalted vital action, but of impaired nutrition, manifested by alternate febrile excitement and cool surface, and pain. These are best treated by gentle anodynes and nutrients. In this stage, under the idea that some organ is exposed to destructive inflammation, a resort to bloodletting is opposed to correct principles of practice.

These remedies will often succeed: nature alone often cures. Overdosing is to be avoided. Calomel largely given in collapse often embarrasses the stage of reaction. Secure the best possible hygienic conditions, remove the patient to an elevated and well ventilated locality when possible, avoid crowding, and secure cleanliness by the removal of dejections of every kind, and immerse all contaminated substances in hot water, or burn or bury them. The cramping may be relieved by the internal or external use of chloroform, and vomiting will often yield to a few drops given inwardly with minute doses of morphine. While quiescence of the stomach continues, brandy and water iced, and iced milk, may be given. It is sometimes necessary to control the motions of the patient in order to retain the heat about the body. Self-possession, on the part of physician and attendants, is necessary to calm fear and give confidence. I would prefer to wait and interpret nature, rather than to use officious medication. This disease is propagated by cryptogamic causes; hence is not contagious, and medical men should not add to the existing fear by calling it so, and creating a panic. This is discreditable to our noble profession, whose boast is to save life and mitigate suffering.

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THE RADICAL TREATMENT OF INGUINAL HERNIA:

AND REPORT OF A CASE OF EPILEPSY CURED BY TREPHINING.

BY JAMES THOMPSON, M. D., MOSCOW, IND.

Having met with quite a number of cases of inguinal hernia in the regiment to which I was attached, and having but a poor supply of trusses or mechanical appliances for supporting the same, and not having Wutzer's instrument, I determined to procure adhesive inflammation, in and around the inguinal canal, by more simple means.

The apex of a Minnie ball was rounded off, and a hole bored through it longitudinally for the transmission of a double annealed iron wire, (silver wire not being at hand). A wooden canula was made out of part of one of the urethral syringe cases furnished by government, and a hole made through its end. A button of lead was hammered out, and two holes were made through it for the purpose of fastening the ends of the wire. A case of hernia was placed in hospital, and a dose of sulph. magnesia administered. Early the next morning, the parts being shaved, the bowel was returned, a portion of the scrotum pushed into the inguinal canal by the index finger, with its palmar surface upward; the canula passed behind the finger, and when found to be well adjusted in the canal, held by an assistant; a common curved needle, armed with the wire to which the ball was attached, and held by a pair of sequestrum forceps, was passed through the canula, made to pierce the scrotal integument and abdominal parietes, and brought out about

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the internal abdominal ring. The canula was then split by bone forceps, and the ends of the wire passed through the holes in the leaden button, drawn tight, and fastened. Half a grain of sulph. morphia was administered, and cold water dressing applied.

The twenty-first day after the operation the ball was removed; but had to be cut out anteriorly, owing to adhesion having taken place in the canal behind it. A compress was placed over the inguinal canal, and the patient allowed to sit up occasionally.

The following day six cases were operated on in the same manner; but in these the ball was allowed to remain but two weeks. With two exceptions, these patients were walking about the hospital in six weeks from the date of the operation.

One failure was owing to the breaking of the wire, from inflammatory swelling, on the fourth day subsequent to the operation. He refused to submit to a second operation. The second was attacked, on the fifth day after the operation, with violent rigors, vomiting and great pain in the abdomen. His pulse was 150; his knees were drawn up; his countenance was anxious; and nearly all the symptoms of peritonitis were manifest. Opium was freely administered, which seemed to relieve him for a few days. On the fifth day he was again attacked with violent pain in the abdomen and vomiting; his pulse was very weak and quick, and his skin covered with a cold sweat. I was at a loss to account for the sudden sinking, but ordered alcoholic and ammoniacal stimulants with opium. He died the following day. I examined the body two hours after death. The omentum was injected and of a greenish hue; a great amount of coagulated blood was in the peritoneal cavity; and upon removing the bowels, a large aneurism of the anterior portion of the abdominal aorta was discovered, in which a rent was found of one and a half inches, through which one gallon of blood had passed. The blood was so firmly adherent to the spleen and left kidney that it was almost impossible to detach it, which convinced me that the rupture of the aneurism must have existed for a considerable length of time. The inguinal canal was found sufficiently closed around the spermatic cord to prevent any return of the bowel. Had the case occurred in civil life, and a post mortem examination been refused me, I should always have entertained the opinion that the man had died of peritonitis caused by the operation.

I exhibit instruments similar to those used in these operations; but instead of the wooden canula I would recommend a curved metallic one, much smaller, made in halves, and fastened by a sliding ring; and instead of the common curved needle, a long one with an eye near its point.

The less weight and cost of these instruments, than those of Wutzer, and the inconvenience of having the latter hanging to a patient for two weeks, are apparent.

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Seven cases of hernia were operated upon; five of which were cured and returned to duty; one failed, owing to the breaking of the wire on the fourth day, and his refusing further treatment; and one died of aneurism.

A CASE OF EPILEPSY CURED BY TREPHINING.

Thomas Jefferson, private 4th U. S. C. Artillery, aged forty-two, eight years prior to enlistment was struck upon the head, causing a fracture near the mastoid portion of the temporal bone. He states that he was 'knocked dead' for some time, and unable to work for three months, when he had a 'fit;' and after about eighty days a second; and he soon had monthly paroxysms. He was treated by several physicians, but benefited by none.

In October, 1864, he presented himself for a discharge; and the signs of an old fracture were distinct. The fits never varied more than two days from being monthly, and were preceded by sickening epigastric pain and drowsiness for one or two days.

I saw him in the latter part of February, 1865, with a true epileptic convulsion, and trephined him directly over the seat of fracture. A spicula of bone was found pressing upon the dura mater; and removed. The wound was closed with silk suture, and cold water dressing applied. He did well until the third day after the operation, when he had delirium, caused by drinking an ounce of whisky. A few ounces of blood were taken from his arm, and he became calm in a few hours. He had no other medicine except a dose of sulph. magnesia, until suppuration had fairly set in. Nitrate of silver was given him for two months. The scalp healed in two months, and October 2, 1865, he was on duty, and had been for several months; having had no return of the disease since the operation.

Is trephining as dangerous as is taught in text-books?

Fifteen cases of removal of bones of the head fell under my notice during the siege of Vicksburg, only one of which proved fatal, and that from intoxication, three months after the operation.

I think few die from the effects of trephining, but many from the effects of the injuries for which the trephine is used.

I am convinced that adhesive plaster should not be used in recent wounds. It does not bring the edges as accurately together as sutures, especially in crucial incisions of the scalp; it does not permit a thorough inspection of the wound; and leaves a gummy deposit, which is removed with difficulty.

But two cases of erysipelas of the scalp after operation have come under my notice, one of which occurred where adhesive plaster had been used.

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EXCISION OF BONE.

BY JOHN A. COMINGORE, M. D., INDIANAPOLIS, IND.

If there is one feature more striking than another which characterizes the surgery of to-day, it is the preservative efforts made to avert deformity, and lessen mutilation. The great practical question which now engages the profession is to what extent the operation of excision of bone is a substitute for amputation. This question has been ably discussed by many of the best minds in the profession, and results and inferences arrived at satisfactory to the energetic men who pioneered the way, and most encouraging to those who have to follow. The interest in the subject has become general; every periodical conveys information from surgeons of eminence who are practically working out this scientific problem; and the retrospect is satistactory and encouraging.

Excision of joints, on account of disease, is not an operation of modern origin. It dates back to 1762, and was performed first by Dr. Filkin; and was again performed and more nearly perfected by Prof. Park, of Liverpool, in the years 1781–2. The result not being what was desired, the operation was neglected until about the year 1850, when it was revived by Mr. Ferguson, and through his efforts made quite successful, and more acceptable to the profession.

Excision of bone, in gunshot injuries, was brought to the notice of the profession during the Schleswick-Holstein war, and its merits fairly tested by Stromeyer, Baudens and other surgeons of professional reputation. Our late civil war furnished ample and sufficient opportunities for further tests

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and experience; and statistics, so far as I have been able to gather them, establish it as an operation, in this class of injuries, of undoubted utility. In these cases, where nature can not effect restoration, excision and amputation come to her relief. The one or the other is to be preferred, according to the character and extent of the injury, and the circumstances surrounding the patient. If life can be saved as well with the preservation of the limb, excision is preferable to amputation.

The statistics of mortality attending these operations show a slight difference in favor of amputation. In army practice this is probably owing to the fact that excision is not an operation well adapted to field surgery, on account of the absence of facilities necessary to effective treatment. A large majority of the cases of excision of the shoulder joint, which came under my observation during the war, progressed favorably—the soft parts uniting primarily—until about the sixth week, when reparation was interrupted and the patient became pyæmic and died. Under more favorable circumstances than the army affords, where the arm could have better mechanical support, the patient be better nourished and nursed, and the wound have the attention requisite to prevent the generation and absorption of poisonous secretions, more cases would recover. The shoulder, knee and hip joints are peculiarly liable to pyæmia after excision; hence every practicable precautionary measure should be used to prevent it. The patient should have a liberal supply of easily digested nourishment, and the wound should be continually drained, and its temperature controlled by cold applications. In the treatment I desire to express my disapprobation of unnecessary handling and pressing of the wounded parts, and washing pus from granulating surfaces. Perfect rest of wound and limb is as necessary in cases of excision as of fracture or amputation. Pus, when formed and collected about the wound, should not be interfered with, except when in excess, or in a state of decomposition, as it serves to protect the raw surface, and in this way favor the healing process.

In military practice excision of the knee and hip joints, in their results, were so very unsatisfactory that few surgeons would perform them in the field. Under favorable circumstances, where the necessary appliances and means for treatment can be had, I regard these operations as practicable and judicious, and would expect favorable results.

In performing excision, the surgeon should carefully preserve the periosteum, as it is now pretty well established that we depend on it for the reproduction of bone. The reproduction of bone, in cases of excision, is important, as it adds to the usefulness of the limb.

It is the general opinion of surgeons that a gunshot wound of a joint, where the capsular ligament is penetrated, and the head of the bone fractured, demands primary operation. My experience corroborates this opinion.

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During the war many cases of this character came under my observation, and proved conclusively its necessity in the field. Quite a number of cases were left to nature, but I am unable to record a single recovery, though in some the injury to the bone was slight.

The results from excision of the knee were so unsatisfactory that few surgeons could be found who would perform it in the field. In private practice, with proper care and management, I think it might be followed with better results. Mr. Butcher's work on Conservative Surgery contains a table of statistics of thirty-one cases of excision of the knee on account of disease, in which excellent results were attained. Out of this number twenty-five recovered, of whom seventeen had perfect use of limb. This degree of success has not followed excision of the knee in cases of gunshot injury; and it is yet to be determined whether the operation is applicable to injured, as it is to diseased, joints.

The elbow has been excised oftener and with better results than any other joint. This success is owing to the dependent position of the joint which favors the escape of the secretions of the wound, and prevents pyæmia. There is not the same risk to run in leaving injuries of the elbow to nature for repair that there is in the large joints; and unless the injury is extensive to both solid and soft tissues, I would recommend that the case be left for future consideration, as time will more clearly define the extent of the injury and suggest the operation.

As a prime object in excision of the elbow is to leave as useful an arm as possible; and as motion of that joint, if ever so slight, adds much to its usefulness, passive motion should be practiced so soon as acute symptoms will permit, and continued until all danger of true anchylosis has passed. This will leave it with more or less motion.

Leaving excision of joints, I now inquire when excision of the shafts of bones is necessary, and to what extent it is required?

Until recently the opinion prevailed among surgeons that gunshot injuries of the shafts of bones always demanded primary operation. This opinion was inculcated by Dupuytren, and was by surgeons generally regarded as correct. During the European war of 1849-50, experience indicated the irrationality of the practice, and it was to some extent discarded. But it remained for the surgeons of the late civil war on our own continent to complete the observation, and decide the impropriety of primary operations.

In the beginning of the war, in injuries of the shafts of bones, it was the practice, not only to perform primary operations, but, in cases of excision, it was the custom to saw off the bone above the comminution, as well as extract the fragments. This practice was not long tolerated; observation and experience very soon proved that it was not only useless but hurtful, as nature was usually competent to repair such injuries without the aid of the

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knife and the saw. The practice of sawing off bone above the comminution arose from the belief that fissured bone would not heal; that necrosis and pyæmia would follow, and the patient die. But experience has proved that idea to be a mere chimera. Many examples can now be furnished which would satisfy the most sceptical that fissured bone will heal, and quite as readily as fractured bone. In fact there can be no surgical distinction. I have a specimen, furnished me by Dr. J. M. Kitchen, of this city, in my possession, of a fractured humerus, very much shattered, that establishes this fact beyond cavil; and this is only one of many that might be produced to complete the evidence. The provisional callus was early thrown out, prior to permanent union, and had no complication arisen which demanded an operation, I am of the opinion that union would have been the result. But on the seventeenth day after infliction of the injury secondary hemorrhage from the brachial artery set in, and the arm was amputated. It was buried nearly four years, and yet presents unmistakable evidence of nature's efforts at reparation. From this I conclude that gunshot injuries of the shafts of bones, when uncomplicated and not too extensive, do not require primary operation further than to extract loose fragments that present, cleanse the wound of any foreign substance, and dress the limb as in compound fracture. Further than this I think the surgeon is not justified in going. If doubts exist as to the propriety of primary operation, let the patient have the benefit of the doubts, and leave the case for future decision. A fact quite well established is, that bone chiefly draws its support through the periosteum. If the attachment of the fractured bone to this membrane is sufficient to keep up vitality, the probability is that union will take place. This at least is a sufficient reason to defer the operation, as it is impossible to decide in a primary examination what part of the fractured bone should be extracted and what left. Time only can correctly decide the matter.

The treatment after excisions should be conducted on the same general principles as that of other similar operations; keeping prominently in view the necessity of controlling inflammatory action, to prevent, so far as possible, its local destructive character. No decided results will be obtained by other than hygienic treatment. When the constitution of the patient is vigorous, venesection may safely be practiced; but he will probably do just as well without it. If left alone, nature will very soon adapt herself to circumstances and correct any existing derangements of the system that are detrimental to the progress of the case. We need not fear hypernutrition of the general system. The injury produces such a shock that digestion is suspended—cutting off the source of nutrition—and it is only restored when the system demands a renewal of nutrient material.

Upon a careful investigation of the subject of inflammation, I am led to think that it is unnecessary to the reparative process. I regard it as a conse-

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quence of the injury, but not essential to repair. The idea that it exerts a peculiar influence over the blood, rendering it plastic, and furthering the growth of healthy granulations, is certainly untenable. A wound of the tissues having been received, the nutrition is interrupted, and nature, true to her office, adapts herself to the change, and heals the breach by gradually reëstablishing normal nutrition. There is always hypernutrition in and about a wound. By this I mean that the tissues involved, that were competent to dispose of nourishment necessary to their support in health, are by the injury rendered incapable of appropriating the necessary amount, and inflammation arises as a consequence. In other words, they are taxed beyond their power. Such a condition would appear to favor general depletion; but in severe injuries it is necessary to economise the strength of the general system, nnd address depleting measures to the injured part. For the successful treatment of wounds we should look more to local than to general application of our remedies. Hyperæmia should be reduced and kept so until the nutritive process is thoroughly established. In this way inflammation is controlled, and its destructive character prevented. To effect this, cold applications, rest, position, mechanical appliances and the ligation of arteries are the most reliable agents. These are to be used singly or in combination, as the case indicates. Bandaging the limb neatly and firmly, from its extremity to six or eight inches above the injury, with dry cold early and constantly applied to the wound, will usually prevent local hyperæmia and control inflammation. Ice should be applied in bladders, or, what is better, gutta percha bags, as it is important that the wound be kept dry. The wound ought to be thoroughly enveloped with several thicknesses of cloth, and the ice applied external to that. An objection to the constant use of ice, by some, is that there is danger of producing so low a degree of temperature that it will interfere with the nutrient changes necessary to the healing process. Experience does not sustain this objection, but proves conclusively that it favors the healing process, since primary union usually follows its use. By the use of ice, incised wounds of the knee, penetrating the joint, are treated successfully, with preservation of the motion of the joint. In case these means fail, ligation or compression of the main artery leading to the wound is recommended; and its advocates believe that it will most certainly effect the object desired—that of subduing inflammation. The evidence is now sufficient to claim the attention of surgeons to this plan of treating inflammation. The method is not new, and before this time should have been appreciated by the profession.

Ligation of the femoral artery in a case of penetrating wound of the knee joint, with the view of preventing destructive inflammation and saving the joint, was performed first by Dr. Onderdonk, of New York City, in the year 1813. The inflammation was subdued and the motion of the joint preserved.

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The next operation was performed by Dr. David L. Rogers, of the same city, in the year 1824, under somewhat unfavorable circumstances. The case was an incised wound penetrating the knee joint. Inflammation ran high and amputation appeared inevitable; but it was thought proper to try conservative treatment. The femoral artery was ligated, and recovery was speedy and complete. The operation has been performed repeatedly since, with the same object and with like results.

The opinion prevails to some extent—theoretically perhaps—that the ligation of a large artery tends to produce a gangrenous state of the wound. Experience not only proves the opinion erroneous, but that the ligation of arteries is a successful method of curing gangrene.

A limb in a passive state requires but a small amount of nutrition to maintain vitality; and, by the time the wound is adapted to, and capable of, appropriating a fuller supply of nutrient material, a collateral circulation will have been established and supply all necessary wants.

In the April number of the *Richmond Medical Journal*, in an article on this subject, Dr. Daniel F. Wright illustrates the effects of the operation with five cases of his own, and six from Dr. Campbell. He claims—"that the ligation of the principal artery of a member, which is ordinarily supposed to occasion danger of gangrene and necrosis in the parts supplied by the occluded artery, has, on the contrary, a marked therapeutic influence, not only upon tumefaction and unhealthy discharges, and indolence in the healing process, but especially upon gangrene itself, which repulsive and dangerous disease it has uniformly and promptly arrested, in every instance in which it has been applied." In all these cases reported by the author and Dr. Campbell this uniform result was observed—that immediately from the date of the ligation large tumefaction was superseded by restoring original contour, fetid and ulcerous discharges gave way to laudable suppuration, and phagedenic gangrene to vigorous granulations, resulting in the rapid detachment of the eroded tissues. In all these instances the ligation was for secondary hemorrhage; but the effect was so marked upon the gangrenous condition, that the question arises, whether it may not be resorted to expressly for this purpose. It proves one point very clearly, that is, we need not hesitate in ligating arteries, fearful that gangrene will result. Dr. Wright requests that whenever an artery is ligated to cure gangrene, the result should be carefully recorded and published that we may see whether experience in the future corroborates and harmonizes with what has been stated.

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VAGINAL FISTULES:

A FEW WORDS UPON THEIR ETIOLOGY, AND UPON SOME OF THE PAST AND PRESENT METHODS OF TREATMENT.

BY THEOPHILUS PARVIN, M. D., INDIANAPOLIS, IND.

Our professional advance in recent years has been greater in no department of practice, than in that which embraces the treatment of the medical and surgical diseases of women. This general assertion, which we believe will be readily accepted, finds one of its best illustrations in the history of methods of remedying vaginal fistules; such history revealing an advance from occasional to almost constant cures, gives testimony that is indeed conclusive. The literature of this subject is scattered through two centuries, though of course much more abundant and valuable in the last fifty years, than in the preceeding one hundred and fifty. It is found in detached fragments in the periodicals and permanent volumes of our profession, but complete in none. I mean by this last to say that there is not, so far as I have been able to ascertain, in the English, French or German language, a complete history of the different methods of treatment that have been devised and executed for the remedying of these lesions. Probably the best epitome is found in the work by Professor Günther, Drs. Schmidt, Hennig and Berger, entitled *Lehre von den Operationem Am Becken Des Menschlichen Körpers*, published at Leipzig and Heidelberg, 1860; but satisfactory as this may be as to continental operators, we find it lamentably deficient as to American and Brit-

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ish—but a very few of each mentioned—Mott, Rhea Barton, Hayward, Minturn and Warren among the former, and Guthrie, Simpson, Collis and Spencer Wells among the latter. In consequence of the absence of such complete history, various inaccuracies in reference to facts and dates, involving questions of priority or of originality, find place in reputable volumes. Some of these errors will be pointed out in the course of this communication, while others I intend publishing elsewhere at a no distant day.

By vaginal fistules we mean communications between the bladder and the vagina, the urethra and the same, and the rectum and the same, constituting in order, and this too is the order of relative frequency—*vesico-vaginal*, *urethro-vaginal* or *recto-vaginal* fistules. In rare instances the ilium either as congenital condition, or as the consequence of disease or injury, has an opening into the vagina, constituting an *intesto-vaginal* fistula. The great rarity of this fistule, and its causation and cure both being generally different from those of the others, will render quite proper its exclusion from our present consideration.

The word fistula, meaning primarily a reed or tube, but illy expresses, in its strict etymologic sense, the physical characters of these lesions, and hence has been justly criticized, especially by Jobert and Vidal; these openings are not tubes or canals, they are gaps, rents, complete solutions of continuity, partial destructions of dividing walls. Nevertheless the nomenclature of the disease is so well established, that it is useless now even to suggest any change.

There is neither time nor need to dwell upon the injurious physical results of these fistules, nor upon the more painful moral consequences to their unfortunate subjects, utterly excluded from social pleasures and social duties. Nor are examples of these disorders exceedingly rare; I happen to know of three cases in one town which has a population of less than two thousand. Women are disposed to be very reticent even to those most intimate of their own sex, unless they should chance to be similar sufferers, in reference to any defect or disease of any portion of their sexual system; one who has thus suffered a long time might tell you of half a dozen others who suffered likewise, while a dozen who were well, could not inform you of one who was ill.

But what are the causes of vaginal fistules? Cancerous ulcerations may produce them, while it is by no means certain that venereal will, though such results have been attributed to them; abscesses forming in the anterior, or posterior wall of the vagina; the very awkward use of the catheter or of craniotomy instruments; puncture through the vagina of the distended bladder, mistaken for the fœtal membranes; cutting instruments in operations upon the neck of the womb; the extraction of the head of the child after craniotomy has been performed, the oftentimes ragged edges of the bones tearing the surfaces with which they are brought in contact; the distension

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of the bladder or the presence of stone in this organ at labor, are among the causes. One word as to the last: In Dr. Baker Brown's work on Surgical Diseases of Women, 2d edition, London, 1861, p. 142, the following passage occurs. "This case is interesting as showing the mischief sometimes done during labor by the presence of a stone in the bladder, a circumstance not mentioned, to my knowledge, by any author." Somewhat less than thirty years before, (see London Lancet, 1836,) Dieffenbach refers to a fistule thus caused; so that Mr. Brown's case of this kind was by no means the first mentioned.

But the most frequent cause of the injuries we are considering, especially of such as are remediable, is prolonged labor, and the resulting continued pressure of a part of the fœtal head upon some portion of the vaginal wall anterior or posterior. When we recall the conformation of the true pelvis as well as that of the fœtal head; when we remember the relative lengths of the vagina in front and behind, the latter greater at any rate, and increased by labor so as to measure eleven or twelve inches; and remember likewise that occipito-anterior are much more frequent than occipito-posterior positions; and, finally, when we study the mechanism of labor, as it relates more especially to rotation upon anterior inclined planes under the pubic arch, and the subsequent fixedness of this portion of the cranial ellipse, while an arc of a circle is being described by the posterior—all these things, properly weighed, we readily understand why the fistule is much oftener of the bladder, or of the urethra, than of the rectum. When we consider that the pressure is not uniform—can not be so from the shape of the fœtal head, and from the differences in pelvic diameters—but is most strongly exerted upon some particular point, we know why the fistule is, as a general thing, limited in extent. Now what results from such continued pressure? The part thus pressed upon is drained of vital fluid—no more can replace the loss, as all access is cut off, and hence death. Subsequently nature establishes an inflammation to get rid of dead tissue; puerperal vaginitis, unless it is exceptionally by possibility, does not cause fistules; it only reveals the destruction which has occurred. It would seem needless to be so explicit upon this point; nevertheless, in a volume of much merit recently published, we are told that *vesico-vaginal fistulæ* frequently result from puerperal vaginitis. If 'fifty per cent.' of these fistules are preventible, as this author teaches, by means used after delivery, then the question is not one of merely abstract and useless opinion.

But what do other authors teach?

Desault gave essentially the view as to the causation which I have repeated here. Vidal says, (*Traite de Pathologie Externe*,) "The cause much most frequent, most potent of vesico-vaginal fistulæ, is the pressure of the head or other part of the child upon the bladder of the mother during a

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long and laborious accouchment. This pressure determines a mortification of a part of the bladder, and of the corresponding part of the vagina, with an inflammation more or less intense of the surrounding parts." Jobert in his classic volume, *Traite des Fistules, &c.*, in his article on the *etiology of vesico-utero-vaginal fistules*, remarks—and the observation, if true in reference to these, is equally applicable to vesico-vaginal or recto-vaginal fistules —"These grave lesions are nearly always the result of a gangrene, determined by the compression of the child's head, or by the action of instruments which have been used in order to extract the infant." In Scanzoni, (Dr. Gardner's Translation, p. 503,) we are taught that "the head of the fœtus, long retained in the pelvis, strangulates between it and the pubes, the anterior wall and the base of the bladder, and provokes in these parts an acute inflammation, followed by gangrene. Thus is formed an eschar of more or less extent, which at length falls off and leaves an urinary fistule." Our only remark upon this is that the "strangulation" is the commencing gangrene, and that inflammation ensues solely for the wise end of separating the living from the dead. Prof. Simpson states that vesico-vaginal fistula "has been attributed, first, to the long continued pressure of the fœtal head on the maternal passages, producing mortification and sloughing of the vagina and part of the wall of the bladder; and secondly, to direct injury from the use of instruments;" and subsequently refers to the former, (though the words are somewhat changed and not quite so explicit,) being the much more frequent cause.

It is scarcely necessary to continue the discussion of this part of our subject; enough testimony has been adduced, I believe, to cast some doubt upon, if not entirely to discredit, the creed that 'fifty per cent.' of the cases of vesico-vaginal fistules which now occur might be avoided by any means that can be invoked after labor, still less by the simple resort to the catheter every four hours in cases of 'puerperal vaginitis.'

A very brief consideration of some former methods of treatment, and of some points as to the present, is now proposed. Vidal states, in the volume to which reference was made, that the history of these lesions does not really commence until J. L. Petit, and asks whether the silence of preceding surgeons did not result from their belief in the incurability of the malady. Vidal was mistaken in attributing such date to this history, for before Petit was born Roonhuysen (1663) had suggested the two most essential parts of the operation, as we this day, more than two centuries after, practice it, to-wit.: paring the fistulous edges and bringing them together with sutures;* and Völter, of Wurtemburg, when Petit was but fifteen years of age, and therefore, it is to be presumed, not contributing largely to surgical science, in

*The suture he advised was the interrupted.

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1679, put in practice the plan advised by Roonhuysen, adding the introduction immediately after the operation, of the catheter, and its retention until the removal of the sutures. Völter, too, first pointed out the fact that transverse fistules are more readily cured than others. Fatio, (1792,) of Basle, was the next to operate, (using twisted sutures,) according to Roonhuysen's plan; he operated upon his patient in the lithotomy position, which Baker Brown (*op. cit.* p. 131), states was 'proposed by myself'!

The next step, though hardly to be regarded as an advance, was the substitution for the suture of the tampon. This was practiced by Petit and Hilscher, and it continued in vogue until the end of the last century, and, it is said, was especially successfully used by Desault and Chopart. The use of the catheter was conjoined with that of the tampon. A successful result, which we must believe exceptional, generally required from six months to a year! The disused suture was again brought into notice by Lewzisky in 1802, and by Nägele in 1812: each recommended it, but neither used it. Nägele suggested the *bistouri cache*, for denuding the fistulous edges, instead of curved scissors. In 1817 Schreger operated, using scissors, however, for resecting the edges; he appears to have used two forms of sutures—one the glover's suture, in the other the threads were conducted through a series of very small wooden balls, and fastened finally to the last ball in the series—an arrangement which would somewhat resemble the shotted wire suggested by Dr. Sims, and still used by some operators. In 1818 Cumin had a case in which the continuous use of the catheter alone effected a cure; and Guthrie, in 1823, had similar success with the catheter, the vagina having a sponge introduced into it; the treatment occupied six months. Then came an age of cauterization: tincture of cantharides, lunar caustic, *lapis infernalis*, acid nitrate of mercury and the actual cautery were used, but undoubtedly with a much greater number of failures than of successes. One of Dupuytren's patients was subjected to the actual cautery twenty-one times in ten months, and no cure. Some, as Failifer and Breschet, conjoined with cauterization approximation of the fistulous edges by means of hooked forceps. Malagodi, of Bologna, succeeded, in 1829—resecting the fistulous edges, and uniting them with the twisted suture. Of Lallemand's '*sondeerigne*' the most that can be said is that, according to the testimony of Serres and Vidal, all the cases operated on in public by its inventor were failures; not a shadow of success attended its use; and no one can see why it should succeed any better in private than in hospital practice, although successful cases were reported.

I believe the first plastic operation for vesico-vaginal fistula, was performed in 1830 by Dieffenbach and it was finally a success. Dr. Churchill states in reference to 'elythro-plastie' (*Diseases of Women, 5th edition*, 1864, *p.* 741,) that Velpeau suggested this operation, and it was first performed by

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Jobert. But Jobert's first case was in March, 1835, five years subsequently to Dieffenbach's.*

There is not time to speak of Jobert's operations, important as they are, or of Wutzer's,† or of Simons', each possessing important original points; or of the methods pursued by various other operators, and the measure of success which has attended their procedures. To a single French operator only will I refer, and then present some of the important contributions made by some of our own countrymen; Vidal (*op. cit.*) describes what he terms the indirect method of treating vesico-vaginal fistula, is case there has been such destruction of the vaginal wall, that it is impossible to close the rent; this method is the closure of the vulva. In the volume to which reference was made, in a former part of this paper, as containing what we believe an utterly untenable theory as to the production of 'fifty per cent.' of these fistules, we find that closure of the vulva 'was suggested, if not practiced by Dr. Sims.' This is a mistake, Dr. Sims may have done the operation, though I find no record of it; but certainly it was neither original in suggestion nor in execution, for Vidal performed it in 1832.

In our admiration for the achievements of Drs. Sims and Bozeman, we are in danger of forgetting two American Surgeons who preceded them. 'There were heroes before Agamemenon,' Dr. Hayward of Massachusetts, and Dr. Mettauer of Virginia; the latter, especially, should not be forgotten. Dr. Hayward's first case will be found reported in the *American Journal*, Aug. 1839; his method was really, though unconsciously, that which was suggested by Roonhuysen in 1663. Dr. Mettauer gives two cases, *American Journal*, 1847, in which he united the freshened edges of the fistula with lead sutures, and the result in one was a complete success. But to Dr. Sims belongs the great credit of the general use of silver sutures, though priority in the use of metallic sutures in the treatment of vaginal fistules belongs, as we have just seen, to Dr. Mettauer, rather than to him. Nay, the priority must rest still further back. In the London Lancet, November, 1834, Mr. Gossett reports a case of vesico-vaginal fistula successfully treated with gilt silver wire sutures.‡

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*See the work by Prof. Gunther and others, previously mentioned, and Jobert (*op. cit.*), as to this question of priority. Dieffenbach obtained his flap from the vagina, the restoration of the integrity of the canal being made with material from the canal itself; Jobert from one or other *labium*, or from the fold between the thigh and buttock.

†Some of the statistics collected by Wutzer are very interesting—such is the following in reference to the success of different methods of treatment: twenty cases of vesico-vaginal fistulæ were subjected to forty-eight operations—among which were elythroplastie, epissorraphie, cauterization, sutures, interrupted or twisted, and both—and only two cured! Such results are in striking contrast with those which are attained by the method now known abroad as the *American*—a title which may be accepted with justice and pride.

‡Baker Brown says 'gold' sutures, but Holmes' System of Surgery and the Lancet both state silver-gilt.

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However, we should not forget that Dr. Physick, from observing the innocuousness of lead when imbedded in the animal tissues, suggested the use of ligatures made of this metal; and that Dr. Levert, of Mobile, acting upon this suggestion, in the year 1828, made some interesting experiments—detailed in the first number of the *American Journal of the Medical Sciences* for 1829—in which he demonstrated some of the advantages of tying arteries with metallic ligatures, in preference to those of gum elastic, silk or grass—the metals experimented with being lead, gold, silver and platinum. But the great practical lesson taught by Dr. Levert's experiments, he himself, still less the world, did not apprehend. The facts were all forgotten until our distinguished countryman, Dr. Sims, a man not less remarkable for his ingenuity than for his resolution and indefatigable industry, demonstrated by numerous successful results the great value of silver sutures in the treatment of vaginal fistules, giving them such just preëminence that even French surgeons, the slowest of all men to learn from any one not of their own country, are beginning to acknowledge it. (See notice in the last number of the British and Foreign Medico-Chirurgical Review of a recent French work on *Genito-urinary Fistula in the Female.*)

To Dr. Sims belongs the credit of having called the attention of the profession to the advantage of what has been termed the quadrupedal position on the part of the patient; that is the pelvis elevated and the shoulders much depressed, so that 'the pelvic and abdominal viscera all gravitate towards the epigastric region, the atmosphere enters the vagina, and there pressing by its normal force soon stretches this canal to its utmost limits, affording an easy view of the os tincæ, fistula &c. 'This position,' says Dr. Gardner, the translator of Scanzoni, 'was discovered by Dr. Sims in 1845.' I will not question its entire originality with Dr. S., nevertheless, priority must again seek another. Seven years before this discovery, Mr. Liston, (see *London Lancet*, June, 1838,) had pointed out the value of this very position.*

The side position, however, as suggested by Dr. Sims, is regarded now as ordinarily preferable to either the 'quadrupedal' or the 'lithotomy.' The duck-bill speculum of Dr. Sims, has become an indispensable instrument, much superior to Gerdy's depressor or Günther's spatula.

Should anæsthesia be induced for the operation?

This is denied by some—(it will be observed that in this paper my chief endeavor is either to present facts not generally known, or else to discuss mooted points)—and there is a positive objection arising from the violent vomiting which frequently succeeds the administration of ether or chloroform,

*Likewise in *South's Chelius*, the American reprint of which was published in 1847, the description of Wutzer's method of operating—this was before 1841—we read that "the patient should be placed on her belly, upon a table covered with a mattress, so that she may kneel near its edge, with her head and chest bent forward, and supported with small bolsters."

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and in consequence of which the sutures tear out, and the rent occurs anew. But when we consider that the suffering is often very severe from the opertion, which is a tedious one, I think we ought, for the patient's sake, to have her anæsthetic. I am sure, too, that few who have operated and have experienced the annoyance and difficulty of vivifying the fistulous edges from forcibly protruding vesical or rectal mucous membrane—protrusions that will occur in spite of the patient's best efforts—will hesitate in the preference between *anæsthesia* and *æsthesia*, so far as the facility, safety and celerity of their work are concerned. By all means, then, let chloroform or ether be given the patient.

Many surgical authors give directions as to the different steps of the operation that will often prove more embarrassing than useful. The fact is that in surgery, as in medicine, he who is a mere automaton, who works mechanically and does everything by rules, will often fail. It is much better in each practice to have well assured principles, and to distinctly recognize the ends to be accomplished; and then the determination of methods, the selection of means and steps, will be wisely made. Hence I do not believe that scissors with a particular curve, knives curved or angular, forceps of a particular construction should be regarded as essential for the operator. Each one must select for himself, according to his own convenience and the special character of the case. So, too, in reference to the extent to which the fistulous edges should be denuded, no absolute rule should be given: the end should be to obtain not only fresh, but even and healthy surfaces. As to whether this denudation, in vesico-vaginal fistule, should be by beveling or by removing a strip of mucous membrane, that, probably, may be best determined by the position of the fistule. In reference to this point it is worth while to present the following extract from *Savage on the Female Pelvic Organs*, (London, 1863): "Although the mucous membrane of the vagina never quits its subjacent attachments enough to form a mucous prolapse, nothing is easier than to detach it by dissection; as the average thickness of the vagina rarely exceeds a line, the beveled resections of the margins of the wound, in the operation for vesico-vaginal fistule, will probably be abandoned in favor of the plan of removing a concentric strip of mucous membrane. If the fistule happens to be in the transverse direction, the former plan can scarcely fail to wound some of the large vaginal arteries, and perhaps the ureters; by the latter there is gained a much larger adhesive surface, with absolutely no risk of the above named complications which so often mar the best executed operations."

Most operators prefer silver wire for sutures, though Prof. Simpson selects iron, mainly on the ground that taking a thread of each metal of equal diameters, the silver will sustain a weight of only nine pounds, the iron a weight of twenty-six pounds. When Dr. Mettauer succeeded with ligatures of

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lead, which really has only one-ninth the strength of silver, it really seems to me that Prof. Simpson's preference is without valid reason; there is no danger of failure from the weakness of properly annealed silver wire.

Various needles have been invented for the introduction of the sutures. The most ingenious of these, probably, is the tubular needle of Startin, as modified by Mr. Hilliard, of Glasgow, one of the best instrument makers in the world. But I must say that after first trying Hilliard's needle, and the broad curved needle* which is probably oftenest resorted to by operators, I much prefer the needle invented by Dr. Emmet, Dr. Sims' able successor in the Woman's Hospital, New York City. The flat needle is objectionable from its making an incised wound; while Emmet's, a small round needle with a slight curve near its point, and armed with silk, to which the wire is attached, makes a punctured wound. After the introduction of the sutures, the edges of the fistule are placed and kept in apposition by simply twisting the two ends of each suture, this twisting being readily effected by a single pair of forceps. Where other means are desired for fixing the wires, probably the best contrivance is the nipple shield of Hilliard, which is a decided improvement upon Bozeman's shield.

Various points of interest in reference to the operation itself, and in reference to the after-treatment, suggest themselves, but to a single one, relating to the latter, will I now refer. It is usually directed by our surgical authors that a catheter should be introduced immediately after the operation, and retained during the process of cure. Dr. Sims has invented an S shaped metallic instrument which is self-retaining.

Here again we protest against a rule. Let us see if we can find a principle. What is the danger from the presence of a small quantity of urine, for example, such an amount as would accumulate in two, three or four hours? Not surely in ordinary cases where there has not been from the original lesion a great loss of substance, or from the long continuance of the malady a great diminution of capacity—vesical distention and thence rupture of the just united edges, nor indeed leakage between them—the sutures ought to be tight enough to prevent the latter, at any rate the inflammatory swelling will. But what we regard as the great danger is not vesical distention but vesical contraction; the former, which may be readily prevented, does mischief indirectly by inducing the latter. What other sources have we of vesical contraction? The urine itself may be an irritant from abnormal composition, or even independently of that. Correct any derangement

*While transcribing this for publication, a friend writes me of very serious hæmorrhage occurring in a case of vesico vaginal fistule, upon which he was operating, immediately upon introducing a suture with this kind of a needle; the loss of blood was so great that the vagina had to be tamponed.

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in its composition; and at any rate give diluents freely so that its specific gravity may be lowered, and it may resemble more nearly warm water, which is one of the most soothing applications that can be made to a painful part. Then we may have small fragments of blood clots and phosphatic deposits which will excite vesical contractions. Now these can be better removed when the urine comes with a gush at intervals of a very few hours, than when it constantly dribbles through a retained catheter. Finally, the catheter itself is in very many cases a source of irritation to the bladder—exciting violent contractions so as sometimes to displace it. I have known, too, the Sims' catheter to turn, (the urethra was large,) dragged in part by the gum elastic tubing attached to it, so that the convexity of the instrument was turned toward the posterior surface of the pubic bones; it is easy to see where its point was, and that that point might, in some situations of the fistule, be forced through.

As the conclusion from all this let me say, that in each case the surgeon must determine for himself as to whether the catheter should be retained or introduced at brief intervals.* No universal rule ought to be given. Where it it is advisable to have this instrument retained, I much prefer the ordinary gum elastic to any other.

It need scarcely be added in terminating this communication, which might readily be protracted to three or four times its present length, that my sole desire and effort in preparing it have been to arrive at truths, historical, scientific or practical; and that while rejoicing in the comparatively recent achievements of our countrymen, and honoring the triumphs of Hayward, Mettauer, Sims, Bozeman and Emmet, I would not deny to those distant, in time or in place, the honor due their labors, or exalt any of the former by giving to them one jot of the merit, or praise of priority in discoveries, which may justly belong to the latter.

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*Since writing this, a friend has handed me the eleventh volume of Guy's Hospital Reports, 1865, wherein Mr. Bryant, in discussing these operations, uses the following language, which, contrary to the rule given by most surgeons, confirms much that I have stated as to the use of the catheter: "The urine should be drawn off periodically after the operation, but it is not necessary to irritate the bladder by retaining the catheter in it." I ought also to add that Dr. Gross, in the 3d edition of his Surgery, uses the following language: "I have lately very advantageously dispensed with it (the retained catheter) in several cases, the urine being drawn off every six hours with an ordinary female catheter."

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HUMAN ENTOZOA.

BY WM. B. FLETCHER, M. D., INDIANAPOLIS, IND.

Three years ago I was appointed to report upon human entozoa. Little did those who selected the subject and the committee imagine the labor and difficulty involved in the production of a report which would convey to the society the important facts which research has developed within the last ten years.

This subject, so important to mankind, both physically and pecuniarily, has not found one laborer in America; and it has remained for the patient German, the clear-witted Frenchman, and the practical Englishman to accomplish our present knowledge of Entozoa.

Three years have been spent upon the subject, and except the hasty sketch in our text books, and the old translation of "Barera on Worms," nothing could be found, after thorough search in this country, which would give your committee an insight into the science of helminthology. At last the literature was sought for by a friend in Europe, and when a year had already gone by, your committee had just prepared to study papers by Owen, Huxley, Ehrenburg, Swammerdam, Leuckart and others. The two large volumes of Küchenmeister and Von Seibold, with all their German detail of the development of all known parasites, vegetable and animal, and Smith's compilation from the French, "Davaine's Human Entozoa," and the ponderous volume of T. Spencer Cobbold, (which is the most recent and complete work,) were carefully examined, from which I have endeavored to converge the real light of the subject into a small space.

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I regret that much which would interest you must remain unsaid; but hope that some enterprising American will present us a well written and illustrated work upon entozoa.

Man is infested with a great variety of internal parasites, and some of them prove exceedingly troublesome. It is evident that a large amount of practical good would ensue if we were more perfectly informed respecting the origin and economy of these creatures; for not only are our personal interests directly affected by their intrusion, but we also suffer indirectly from the injury and destruction they occasion among our domesticated animals.

Entozoology makes us acquainted with the forms, habits, structure, development, distribution and classification of a multitude of evertebrated organisms, which take up their abode at one or more periods of their lifetime in the bodies of man and animals.

The best plan of studying the entozoa is to regard them as a peculiar fauna, destined to occupy a peculiar territory, the interior of man and animals. Each animal, or 'host,' may be regarded as a continent, and each part or viscus of his body a district. Each district has its special attractions for particular parasitic forms; yet neither the district nor the continent is suitable as a permanent resting place for the invader.

None of the internal parasites 'continue in one stay;' migration is the very soul of their prosperity, while their retention in the interior, beyond a certain period, terminates in their death.

That the science of human entozoology has advanced with rapid strides in the last ten years is owing to the improved methods of observation—the first of which is the microscope: the second, that instead of studying entozoa in man, naturalists patiently began with the rudiments of the science and learned the laws of parasitic life among earth-worms, caterpillars, snails and frogs; thus tracing, step by step, a knowledge of the laws of animal life, first clearly enunciated by Gœthe and Von Baer, namely; "that development is always from the general to the special—from the simple to the complex—from the homogeneous to the heterogeneous; and this by a gradual series of differentiations;" or as Tennyson has expressed it—

> All nature widens upward. Evermore
> The simpler essence lower lies:
> More complex is more perfect—owing more
> Discourse more widely wise.

As in the more perfect understanding of the science of chemistry we have had to reduce the number of elementary substances, so a more perfect knowledge of the human entozoa has reduced the number from eighty to thirty-one well marked forms, and the majority of these comparatively rare; yet some are exceedingly common, and numerically abundant upon the same individual.

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It is not a matter of wonder that early observers made so many species, for it is but recently that the laws of alternate generation have been understood, and previously the whole class of entozoa were accounted for on the ground of spontaneous generation, and it was thought that they sprang from undigested food, or were developed in the most widely different organs from corrupt juices: and even now, among many persons, it is difficult to eradicate this belief and substitute the laws of nature as drawn from experience.

It has been by troublesome research and careful experiment that the sexual apparatus was discovered in many parasites, such as round worms, flukes, thread worms and tape worms; in which such an immense number of eggs are generated that it seems unnecessary to look further to account for their origin. But how they obtained admission to the interior of the animals they are destined to inhabit was long but dimly understood, until attention was directed to certain peculiarities in the mode of life of these creatures. At particular periods of their existence the intestinal worms undertake migrations in order to reach that animal whose organs are fitted for their habitation. The young of most of the intestinal worms can not develop into the likeness of their parents within the intestine where they were deposited as eggs, therefore the necessity of migration. What would be the result if the millions of eggs that a single round worm or tape worm can produce could develop and generate their young in the same intestines in which they were laid? Would not the intestines become choked up with their successive broods and thus cause death to the 'host' as well as terminate their own species?

A circumstance, favorable to the progress of intestinal worms, is that the structure of the egg shell effectually protects the enclosed embryo against injury from without, and maintains within it the degree of moisture requisite for the further development of the ovum; and thus they may be preserved for months, notwithstanding the vicissitudes to which they are exposed. They pass into dust heaps, privies, drains, &c., and finally are, as manure, spread upon fields where they become further developed. These young intestinal worms, still almost microscopic in size, have not far to seek an opportunity of entering other animals, as they are scattered among the seeds sowed, and these latter produce plants which serve as food to man and animals; and the young worms, adhering to them, are easily swallowed.

Frequently the rains wash the ova of intestinal worms into streams, wells and springs, thus affording another entrance into man and animals by means of the water which they drink.

The observations upon certain remarkable metamorphoses of intestinal worms constituted a complete chaos, which apparently contradicted the acknowledged laws of animal existence and propagation, until the penetration of a Danish naturalist, Steenstrup, discovered the law of alternate gen-

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eration, which may be thus expressed: "That an animal bears young which are and remain dissimilar to their parents, but bring forth a new generation, whose members, either themselves or in their descendants, return to the original form of the parent animal."

Some writers and physicians to this day confound alternate generation with metamorphosis as exemplified in the tadpoles of frogs and toads, and the larva and chrysalis of most insects.

The difference between alternate generation and metamorphosis is that the young in the first not only differ from their parents at first, but remain so, and give birth to offspring unlike themselves, which may return to the likeness of the grand-parents, or still have another progeny before a return to the original likeness takes place; whereas, in metamorphosis, an animal gives birth to young, which, although not like the parents at first, becomes so before it can reproduce at all.

A familiar instance of alternation of generation, which we can test for ourselves, is the parasitic worm, monastomum mutabile, found under the eyelids of ducks and geese. It brings forth a brood in the likeness of active infusoria, covered with cilia, and moving about in the water with the same gliding motion which we so often observe in other infusorial animals. These embryos develop sacs in their interior, and as they approach maturity hide in water snails, (I have found them in the three most common varieties of our waters); and then the cercaria dies, and sets free the living sacs which are housed and supported by the snail. These cercaria sacs are transparent; have a mouth and digestive tube, and within, young cercaria, which growing, burst their sacs. But they are no more the image of their parents than was the sac to its parent—neither do they resemble their grand-parents, for they have a tail like a tadpole; but the genealogy rights itself by their becoming parasites of a water beetle or fly, which being devoured by a bird, the cercaria is destroyed, but liberates the enclosed monastomum.

CLASSIFICATION.

For convenience and accuracy of description the arrangement by Cobbold is the most complete. He does not break up the helminths into separate animal groups, but retains them in a single class, forming the lowest section of the articulated division of the animal kingdom; because all attempts to distribute the entozoa among the different invertebrate groups, to which they are supposed to be most closely allied, has hitherto been attended with end less confusion; therefore, in the present state of our knowledge it is best to adopt the method which experience has indicated as already existing in nature.

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The classification proposed is as follows:

Class Helmintha.	Sterelmintha, (Sub Class i.)	Order 1.	Turbellaria.
		" 2.	Trematoda.
	Cœlelmintha, (Sub Class ii.)	Order 3.	Nematoda.
	Anenterelmintha, (Sub Class iii.)	Order 4.	Acanthocephala.
		" 5.	Cestoda.

Thus we have in this class three sub-classes and five orders. The first sub-class containing all the solid parenchymatous helminthæ—example, liver flukes; the second, all those in which the internal organs are loosely suspended in a well defined perivisceral cavity—example, the round worm; the third, all those worms which are without an alimentary canal—example, the tape worm.

In a paper of this kind it would be impossible to describe each member of the class of helmintha: without numerous illustrations, and a special glossary containing its peculiar nomenclature, no general practitioner would be enlightened.

That diseases are produced by animal parasites in the various stages of development, is and has been acknowledged by medical men. We know not how many more diseases, now termed obscure, may result from unknown parasites, or unrecognized forms of those already known; and it is only by greater care and attention upon the part of physicians; by examining the excreta of patients; by more minute research in post mortems; and by the study of the parasites of every animal which falls under their observation, that further light can be obtained. Should time or means for observation be wanting, put the specimen in a bottle of glycerine, and keep it for further opportunity, or send it to some investigator of worms. In this way, no doubt, entozoa peculiar to our country might be found, and some diseases better understood. Examination of the parasites found in poultry, pork, beef, and even the vegetables which are used as food, at the same time closely watching the diseases prevalent and the excreta of patients, no doubt would be rewarded by discoveries in the etiology and pathology of diseases now little understood.

As there are human parasites too small for examination with the naked eye—the vibriones and monads which swarm in animal infusions and in the mucous secretions of the bowels and vagina of many patients—we must beg for a more general use of the microscope in this study. There is no department of diagnosis more obscure and uncertain without that instrumental aid; and it is one of those rare cases where the microscope is a positive means of diagnosis. The microscope has not yet been able to indicate a positive difference between white blood corpuscles and pus globules; the micro-patholo-

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gist can not say this is cancer, or that is scrofula or tubercle; it is only an aid by which he arrives at certain conclusions when he has the assistance of all the other points in the case. But in determining the presence of entozoa the diagnosis is positive. The eggs of various worms, and parts of the parasite itself, are almost always to be found in vomited matters, urine or fæces; and when once their forms are familiar to the observer, no further symptom is needed.

The positive harm some worms do in the animal economy is unknown, and great injury is frequently done to children, both by parents and physicians, by the use of worm remedies, when some other substance has produced the symptoms.

ENTOZOA OF THE BLOOD.

Davaine, of France, and some others have discovered certain protozoa in the blood of birds, reptiles and fishes. In Egypt, in 1851, it was discovered that a peculiar trematode worm was a frequent inhabitant of the abdominal veins. When this hæmatozoon is situated in the vessels of the walls of the bladder it gives rise to marked structural lesions: the mucous membrane is studded with numerous circumscribed spots considerably injected with blood, and covered with tenacious yellowish masses of lymph, in which the ova of the worm are imbedded. The ova are sometimes passed in the urine, giving it a sandy appearance.

These facts indicate that we should closely watch the secretions and fluids of the body, even those in the dark canals of venous and arterial circulation. If the microscope has found one kind of hæmatozoa, in Egypt, producing serious disease, may we not find, in America, other forms, producing other injuries? Thus far no part of the human subject has been considered free from them: some are found in apparently inaccessible localities, as the brain, the kidneys and the chambers of the eye.

There is one entozoon, the trichina spiralis, which has attracted much attention in parts of Europe during the last six years. It becomes important to us as it has made its appearance in our country, or, at least, has been recently recognized here. The only scientific body which has given attention to this matter is the Chicago Academy of Science. I need make no apology for quoting from its recent report, as this is the only reference made to any special entozoon. I would suggest that this association appoint a committee to examine into the condition of slaughtered swine and other animals in various parts of our State. I believe that by microscopical examination into the tissues of hogs having died of hog cholera, that disease will be found to be caused by the migration of some entozoon similar to the *trichina spiralis*.

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"THE PROPORTION OF DISEASED HOGS.

"Out of 1,394 hogs examined, 28 were found with trichinæ in the muscles. We may therefore conclude that in the hogs brought to Chicago one in fifty is affected with trichinasis in a greater or less degree. We must confess our surprise at arriving at this result, which indicates with little doubt the startling fact that trichinasis in pork is even more common in this country than in Germany, where it has caused so much suffering and death. For instance, in the city of Brunswick, where a most careful inspection of 19,747 hogs was made in the years 1864–5, only two were found to contain trichinæ in their muscles.

OUR IMMUNITY.

"The comparative immunity from the disease which our own people have enjoyed, undoubtedly results from the habit of cooking meat before eating it, while in Germany it is eaten raw by the poorer classes on account of the high price of fuel.

VARIATION IN NUMBER.

"Specimens examined showed great variation in the number of worms infesting them. We have given, indeed, only an approximation of the number existing in a cubic inch in each specimen of muscle; but this approximation is sufficiently near the truth for our present purpose. Our method has been to count the trichinæ occurring in several different portions of muscles, each a cubic tenth of an inch in size, and to multiply the average number by 1,000 to find the number to a cubic inch. By this method we find that only three of our specimens, out of twenty-eight, contain over 10,000 to the cubic inch, and are, therefore, as densely infested with the worm as the pork which has occasioned the disasters in Germany. The remaining twenty-five are infested in a comparatively slight degree, viz.: from 48 to 6,000 to the cubic inch. The specimens most thickly infested contain 18,000 to the cubic inch, and we have calculated that a person eating an ordinary meal of this pork in a raw state would speedily become a victim to the ravages of not less than 1,000,000 of trichinæ. In certain cases of death from trichinasis, the number found in the muscles of a man has been 2,000,000.

"With regard to the muscles of the hog most liable to be infested, we have to state that our determinations do not accord with those of European observers, inasmuch as more than half of our trichinous specimens have been taken from the spinal muscles. In Europe the microscopic inspectors of pork are directed to examine nine different sets of muscles, viz.: those of the diaphragm, tenderloin, shoulder, front and back of neck, intercostals, extensors of fore arm, flexors of the leg and the muscles of the larynx. In

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this list the spinal muscles are not even mentioned, although their examination with us has proved the most prolific in results, even making allowance that we have searched these muscles more than any others. Our experience has led us to believe that a much smaller number of examinations than are required by the European rules will suffice for the detection of trichinæ in dangerous numbers in pork.

DEFENSE AGAINST ITS RAVAGES.

"Now that the existence of trichinæ in our pork has been established beyond doubt, it will be proper for us to point out all known means of defense against its ravages.

"First, with regard to the rearing of hogs. These animals undoubtedly become infested through the eating of flesh of some kind, since no trichinæ nor germs of trichinæ have ever been found in any vegetable food. A strict attention to the feeding of hogs, and their confinement in pens where no animal food is accessible, is an infallible preventive against trichinasis in them. Such management is all the more necessary, since European authorities agree that it is impossible to diagnose the disease in the animal from external appearances, and no culpability can therefore attach to the farmer for selling hogs which prove affected with trichinæ.

"In regard to pork, the origin of the worms in which is doubtful, the use of the microscope is primarily indicated. With this instrument only can we ascertain with certainty whether the muscles of the hog are free from the parasite. But we have in our power much more simple means of insuring safety in the consumption of pork. It is simply to cook it thoroughly so that every portion of meat shall have experienced a temperature of at least 160° Fahrenheit. We can not insist too strongly on this point. Again, by properly salting and smoking the meat for at least ten days, the trichinæ will certainly be killed. Simple dessication of the meat, for a period of sufficient length, will also kill them. They will never be found in old hams, for instance. Mere pickling appears to have very little effect upon these worms.

EXISTENCE BEFORE DISCOVERY.

"Trichinæ have doubtless always existed in the muscles of the hog, although probably not to the same extent as at present, and trichinasis in man may have existed to a considerable extent in this country before its nature and cause became known.

ECONOMICAL ASPECTS.

"Having now fully exposed the exact extent of the danger from trichinous pork to which our people are liable, and stated the means of avoiding

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it, we will proceed to close our report with a few remarks upon the economical aspects of the subject. A panic has been produced in the public mind by the news which has reached us from Germany concerning the disasters which have occasionally followed the consumption of pork in a raw state. The excitement has, with little doubt, been fostered by interested persons for speculative purposes, until people have come to imagine there is danger in eating pork of any kind. All this excitement has occured before a single instance of the occurrence of trichinæ in American hogs has been, as far as we are aware, authentically reported. It has, therefore, become necessary that the subject should be thoroughly investigated in order that the people, by familiarity with the danger, and confidence in their understanding of its character, may not be the prey of superstitious fears. The panic which now prevails is unfounded in reason, senseless and greatly injurious. The commercial aspect of the question is a matter of small amount compared with the great importance of pork as the kind of meat diet upon which nine-tenths of our agricultural population, north and south, mainly depend. Let the people understand that only one hog in forty-eight contains trichinæ at all, that only one in three hundred contains them in sufficient numbers to cause considerable danger, and that in these cases the worms are rendered innocuous by proper smoking, drying or cooking; and we imagine few sensible persons will refuse pork as food, if it suits their convenience to use it."

Before I submit this report, allow me to call attention to a large class of animals which were once admitted among human entozoa, but are now known as the harmless larvæ of certain insects, or proper entozoa of evertebrated animals; among which may be mentioned the common garden slug, the earth worm, the apple tree borer, and the common hair worm, (so called from its supposed rising from a saturated horse hair). There are patients who are willing to make oath to numerous cases of snakes, toads and lizards having been carried, for various periods of months and years, by themselves or other persons.

During my search for entozoa, a friend sent me a fine fat grub which the patient, who had imagined something wrong in her stomach, had vomited upon the ground one morning shortly after drinking water from a gourd. From the structure of the worm, I doubted its belonging to the entozoa, and upon investigation it turned out to be a larva of the apple tree borer, which as ascertained by accurate experiments, will not stand the action of the gastric juice longer than an oyster.

The common garden slug is another of this class of spurious helmintha; and its ability to stand the action of the gastric juice, the following experiments show: Dr. John C. Dalton refers to the occasional accounts in the medical journals in which various cold-blooded animals, especially slugs and

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lizards, are said to have remained alive for a considerable time in the human stomach or alimentary canal, and details one or two remarkable cases that occurred in his own practice. He points out, however, the difficulties that lie in the way of admitting any explanation of this nature—as, the improbability of an animal so large as a slug or snail having been recently and inadvertently swallowed; and the equal improbability that it should have been hatched from the egg, or grown from a very small size; the injurious and probably rapidly fatal effect of a temperature of 100° to a cold blooded animal; the softness and, as is shown in the case of the oyster, the easy digestibility of the living animal; and the necessity, in the case of the slug or snail, of air for respiratory purposes. Notwithstanding these difficulties, he still thought that some more exact experiments than those hitherto made might be set on foot, and he accordingly introduced living and active slugs into the stomachs of healthy dogs, taking care that no injury was occasioned by mastication, the dogs being killed after the lapse of various periods. No traces of the slugs were found in one instance after twenty-four hours; in another after only one hour; in a third the animals were all dead after the lapse of fifteen minutes. Fresh gastric juice from a dog, at a temperature of 100°, killed four slugs in nine and a half minutes, and at the expiration of five hours they were thoroughly disintegrated. Mere immersion in pure water at a temperature of 100° was sufficient to prove fatal. In another experiment two living water-lizards, *triton mille-punctatas*, were administered to a young dog; in fifteen minutes they were both found in the stomach, dead, soft and flaccid. From the similarity of human gastric juice to that of a dog, there can be little doubt but that the effects would be analogous in the case of similar animals being introduced into the human stomach, and that consequently the cases of their supposed long continued residence in the stomach are unworthy of credence.

In October last, Dr. Clippinger gave me a little bottle containing eight or ten little maggots, which he informed me came from an abscess upon a boy's leg. Although they were much shrunken by the alcohol, I managed to mount two specimens upon glass slides, and examined them carefully, and believe them to be larvæ of the flesh fly, and that the eggs were deposited either in the ulcer or through some opening in the skin. Küchenmeister describes them and relates cases similar.

The last case which came under my observation was one to which attention was directed by reading a letter from Dr. Rufus Haymond, of Brookville, published in the January number of the "Cincinnati Lancet." The communication stated these worms (which he sent to the editor) were vomited by a man in the neighborhood; that the worms were numerous, and of different sizes, supposed to indicate various hatchings of ova in the man's stomach. Dr. Haymond remarked that he took them to be gordii

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aquatici, and supposed they had crawled into a jug of water in the field, and the patient had drank them. The editor remarked that worms had been exhibited like those sent by Dr. Haymond, and were supposed to be the little thread worms of gigantic length.

These paradoxical developments were enough to excite curiosity. First, if Dr. Haymond was right in their being true *gordii*, or hair worms, and had found little ones as well as great, and they had remained thus long in the human stomach, it was contrary to all the acknowledged laws that govern that curious worm; for the hair worm is rarely seen but in adult life, and always nearly the same length—an argument frequently produced to prove them only metamorphosed horse hairs, "because," said the ignorant, "if they come as all other animals do, from eggs, why do we find no little ones?" We learned long ago, from the careful experiments of Von Siebold, that the gordius was a true parasite of beetles, crickets, grass-hoppers and butterflies, and only when ready for sexual maturity did it crawl forth to become aquatic, and deposit its eggs.

The editorial remark of their being gigantic developments of the little thread worms was equally startling, for those little worms live in the rectum and not in the stomach.

The worms were sent for, and by Dr. Haymond's kindness, I was allowed a careful inspection. The bottle was supposed to contain three worms, two of the usual size, male and female; and by a magnifying power of sixty diameters were, without doubt, gordii aquatici, as Dr. H. supposed; but the point which I was most anxious to know was, if it were possible to find a partly grown gordius out of its proper 'host,' but to my disappointment, only a hair from the pubes (of his patient I suppose), was revealed. But their having been in the human stomach, even for a week, or their coming from the patient at all, was to be solved, and I received the following statement from Dr. Barry, who treated the case. (The main question is yet unanswered —were the worms ejected from the stomach?)

"Those you have are good specimens of all I observed—of two colors, probably significant of their gender; and of two sizes, indicative of their ages: mostly living when vomited up, but a part of them dead. We often have some very curious stories from our patients, and probably if we were to pay more attention to them, we would learn much to be true that we in advance disbelieved.

"Dr. Stringham tells us that he had a patient who believed she had some living animal in her stomach; that he ridiculed the idea; but, after a time when she was nearly dead, consented to administer some remedies appropriate to her understanding of the case; and after she had taken the second dose his patient voided a considerable number of animals, not familiar to the family, and the woman got well. The Doctor, who was one of the learned

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of our profession in his day, says they were of the order *vermis molusca*, and of the genus *actina*. He and Dr. Crumpe are the only men I know of who ever saw such animals voided from the human body; and each of them, in his time, believed he had seen what no other man had seen.

"Had I visited my patient the night before his serious illness, and had he told me he had such worms as he afterwards vomited up, I would have paid no attention to his story; or if I had visited him the day after and he had told me what occurred, I should most likely have thought it an honest 'fish story.' That he had the worms and vomited them up is true. (Did you see him vomit them, Dr.?) But how came they there? Just as they got into the stomach of Dr. Stringham's patient, and as the same kind of worms get, severally, into the stomachs of men, pigs and horses—swallowed with the water he had drank. How long they had been in the stomach is uncertain. He was a farmer, and throughout the season had carried water to the field in a jug, which he left sunk in the spring while he ate and rested at noon. So far as I saw he voided no worms in his dejections, which would seem to argue they had been swallowed a short time before their expulsion. This may have been so, as they are of that order that multiply rapidly, and get their growth quickly and die.

"Some think such worms can not live in the gastric juice, or in the stomach; this is most probably an error, as there are but few living worms that do not seem, structurally, as able to resist the action of the gastric juice as the mucous membrane of the stomach. That worms are easily digested when dead, I think quite likely. I have frequently been told by mothers that their children voided worms 'all cut to pieces,' which I have supposed to be remnants of worms that have died in the stomach. This I have noticed particularly to have been the case after the administration of the sulphate of lime as a vermifuge.

"The worms you had, as is the case with all the lowest order of animals, require but little atmospheric air. When put into a small-necked vial nearly filled with water they would occasionally rise to the surface as if to breathe. The boys at the drug store corked the vial tightly, in the evening—in the morning they were dead—died, as I believe, of suffocation; proving that a little air was necessary to their existence."

With this, gentlemen, I close the report, which may convey little that is new to you, but which I trust will awaken an active interest in the subject, and cause each one of you as many pleasing hours of study, to be repaid by as many growing thoughts, as it has your committee.

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PROGRESS OF MEDICAL SCIENCE.

BY H. C. COLE, M. D., KOKOMO, IND.

The laws of disease in man are a part of those laws by which he exists as an organized being; and the laws of his organization are but a part of those which belong to all organized matter.

These truths, which form the basis of our medical knowledge and practice, are among the best results of progress. It they have reduced our hopes in regard to the cure of pathological states, they have improved our etiology and diagnosis of disease, and are guiding us to a more perfect system of therapeutics. Our medical philosophy is not as presumptuous as experience alone would make it, but is reliable from experience justified by reason.

A too vigorous application of such ideas has led into medical skepticism many of our profession, but the great majority have been able to pause within the limits which rational experience prescribes.

Under the light of improved medical science, heroism has given place to a more rational treatment, and nature and art are now united in conducting diseases to favorable terminations.

Nature always attempts to preserve life, and if man had not transgressed the laws of his physical being, the vital force would be his best physician. The tendency of the human mind, however, to believe in remedies is universal, and we are forced to believe has some ligitimate end, from the fact of its existence. How pathological processes can be changed by the agency of medicine, we but imperfectly understand, but the difficulty of this problem has but little to do with our practical duties.

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When we turn from the general principles upon which medicine is based to the numerous sciences cultivated with reference to it, the evidences of progress are manifest. Pathology, a knowledge of which at the present day is all important to the practitioner of medicine, is of comparatively recent date, as a science. Hippocrates was its originator, and seems to have appreciated the value of facts over opinions; but had little knowledge of the structure, and less of the functions of the body. Aristotle indulged in wider speculation, but his ideas of anatomy and physiology were erroneous and fanciful. Praxagoras exulted in the discovery that the arteries were air tubes. Erasistratus and Herphilus, 320 B. C., in Alexandria, are said to have dissected human bodies. Galen, in the second century, first describes most of the organs of the body as they are known at the present time. In two instances he saw the motions of the heart, but did not conceive the circulation of the blood. The history of medicine is darkened by superstition and mystery from this time until the thirteenth century, when the university of Bologna divided its professors into physicians and surgeons, physicians for wounds, barber-surgeons and oculists. In 1315 Mondine dissected the bodies of two females, and asserted that the blood flowed from the lungs to the heart. Matthew de Gradibus, in 1480, dissected and represented truly the ovaries. Vesalius presented the first comprehensive work on anatomy. In 1619 Harvey announced the discovery of the circulation of the blood, and revolutionized medical science. Still later Malpighi made the first investigation of the minute structure of the skin and secretory glands; Leuwenhoeck recommended the use of the microscope; and Ruysch introduced plates into anatomical publications. Morgagni may be considered the father of pathological anatomy, having made six hundred dissections, and embodied his observations in 1682 in his work on the cause and nature of disease. About the same time Boerhave, at Leyden, dissected four hundred bodies, and published his anatomical and physiological plates. In 1788 Desault opened the first surgical clinic, in the Hotel Dieu, at Paris; and about this time Corvisart introduced morbid anatomy with dissections into his lectures; and Dupuytren demonstrated that the physiological laws were applicable to disease; Hunter published his work on the anatomy of the gravid uterus; Laennec introduced auscultation and percussion; Rokitansky verified his pathological views by the examination of 30,000 bodies; and iconographic pathological anatomy, originated in the rude representations of disease suspended in the temples of Esculapius, received that impulse which has developed it until it has almost reached perfection in the photographic representations of disease recently published. In physiology, the discovery by Charles Bell of the double functions of the spinal nerves; by Marshall Hall of the reflex and independent functions of the spinal cord; by Carpenter of the functions of the sensory ganglia at the base of the brain; by Laycock of

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the unconscious action of the cerebrum; and the further investigation of reflex nervous phenomena by Brown-Sequard; and of the functions of the cerebellum by Dalton, have secured great advancement in the treatment of cerebral and cerebro-spinal diseases, and placed insanity among the physical ills of life as a preventable and curable disease.

We have only to point, in the history of medicine, to the discovery of vaccination by Jenner, for an enduring and undisputed evidence of the reality of medical progress; and although much skepticism and superstition still exist, we may console ourselves by the reflection that doubt is necessary to progress. It has seemed impossible for some persons to believe in the reality of progress in medicine, because they are forced to admit the impossibility of bringing it to perfection. We must accept partial facts and probable truths as a basis for further investigation. Meteorology and vegetable physiology are as imperfectly understood as the phenomena attending health and disease in the human body; but the farmer does not, therefore, cease to plant and reap, or add to and improve his knowledge.

It has been said that to philosophize is better than philosophy; and the investigation of medical science, though yet in some respects inconclusive, is far better and more creditable than indifference and supine skepticism. The pursuit of such studies, and the practice of our humane art, not only benefit the race, but are productive of moral elevation. Let us, as this occasion brings back the memory of those who once gathered with us, and we recall the names of many now dead whose lives were made up of deeds of devotion, charity and self-denial, so emulate their bright example as to commend the claims of medical science, strengthen its foundations in the esteem and confidence of the public, and contribute to its advancement in the future.

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CONSTITUTION

OF THE

INDIANA STATE MEDICAL SOCIETY.

ARTICLE I.

TITLE.

The name and title of this Society shall be The Indiana State Medical Society.

ARTICLE II.

OBJECTS.

The objects of this Society shall be, to provide an organization through which the regular physicians of the State may be united in one great professional fraternity, for the purpose of giving frequent and emphatic expression to the views and aims of the medical profession; to supply more efficient means than have hitherto been available for cultivating and advancing medical knowledge; for elevating the standard of medical education; for promoting the usefulness, honor and interests of the medical profession; for exciting and encouraging emulation and concert of action among its members; for facilitating and fostering friendly intercourse between those engaged in it; for enlightening and directing public opinion in regard to the duties, responsibilities and requirements of medical men; and for the promotion of all measures adapted to the relief of the suffering, and to improve the health and protect the lives of the community.

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ARTICLE III.

MEMBERS.

The members of this Society shall consist of delegates or representatrves from the various county and local medical societies of this State, organized in accordance with the provisions of this constitution, who shall serve one year, or until others are elected to succeed them. The present members shall be entitled to permanent membership, with all the privileges of delegates.

ARTICLE IV.

COUNTY SOCIETIES.

SECTION 1. Any county or local medical society, whose constitution embraces the objects of this constitution and the code of ethics of the American Medical Association, shall, upon application, become auxiliary to the State Society, and shall be entitled to one representative for every five members, and one for every additional fraction of more than half this number.

SEC. 2. The names of the members of such county or local societies, with their post office addresses, shall be certified to by their respective secretaries, and forwarded to the secretary of the State Society, who shall enrol them in a book kept for that purpose; and each member shall be entitled to a copy of all the transactions of this Society.

ARTICLE V.

OFFICERS.

SECTION 1. The officers of the society shall be a President, Vice-President, Secretary, Assistant Secretary, Treasurer and Librarian.

SEC. 2. Each officer shall be elected by a vote of a majority of all the members present, and shall serve one year, or until another is elected to succeed him.

ARTICLE VI.

DUTIES OF OFFICERS.

SECTION 1. The President shall preside over the meetings; preserve order; call meetings, when, in his judgment, the interests of the profession may require it; and perform such other duties as custom and parliamentary usage may require.

SEC. 2. The Vice-President shall assist the President in the performance of his duties; and, in his absence, preside over the meetings.

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SEC. 3. The Secretary shall keep correct minutes of the proceedings of the Society, and, when approved, fairly transcribe them in a book kept for that purpose. He shall have charge of all books and papers belonging to the Society, excepting such as may properly belong to the Treasurer and Librarian; receive all moneys due the Society and turn them over to the Treasurer, keeping an account of the same; perform all other duties which the usage of corporate or organized bodies may require; and serve as a member of the Committee on Publication.

SEC. 4. The Assistant Secretary shall assist the Secretary in the performance of the duties of his office.

SEC. 5. The Treasurer shall receive all moneys due the Society, and pay all bills approved by the Finance Committee and countersigned by the President, keeping a correct account, and making a full, detailed report of the same to the annual meetings of the Society; and serve as a member of the Committee on Publication.

SEC. 6. The Librarian shall have charge of all the books, manuscripts, (not specially belonging to the Secretary and Treasurer,) instruments, specimens, preparations and other scientific property belonging to the Society, keeping a complete catalogue of the same; and report the condition of his department to the annual meetings of the Society.

SEC. 7. The officers shall deliver all records, books, papers, funds and other property belonging to their several offices, to their successors when they shall enter upon the discharge of their respective duties.

ARTICLE VII.

STANDING COMMITTEES.

At each annual meeting the President shall appoint the following Standing Committees, each to consist of five members, and to serve until their successors are appointed and enter upon the discharge of their duties, viz.: a Committee on Arrangements, a Committee on Credentials, a Committee on Finance, a Committee on Ethics and a Committee on Publication.

ARTICLE VIII.

DUTIES OF STANDING COMMITTEES.

SECTION 1. The Committee of Arrangements shall, if no sufficient reason prevent, be mainly composed of members at the place where the next annual meeting is to be held, and shall provide suitable rooms and accommodations for the meeting, and in all matters, not otherwise provided for, superintend and protect the general interests of the society.

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SEC. 2. The Committee on Credentials shall examine and report upon the validity of the credentials of delegates from the county or local societies.

SEC. 3. The Committee on Finance shall superintend the monetary affairs of the society, inspect and audit all bills and the accounts of the Treasurer, and recommend the assessment of such pro rata tax upon its members, and those of the auxiliary societies, as may be required to defray the current and incidental expenses of the Society.

SEC. 4. The Committee on Ethics shall examine and report for the action of the Society, while in attendance upon its meetings, all cases of appeals from county or local societies, and complaints against members for non-professional conduct.

SEC. 5. The Committee on Publication, of which the Secretary and Treasurer shall be members, shall have charge of preparing for the press, and of publishing and distributing such of the proceedings, transactions and memoirs of the Society as may be ordered for publication; it shall supervise and edit all papers presented to the Society and ordered to be published; and report its doings to each annual meeting.

SEC. 6. The Standing Committees shall keep regular minutes of their proceedings, and furnish an authenticated copy thereof, to be deposited with the Librarian.

ARTICLE IX.

VACANCIES.

All vacancies in offices, occurring in the interim of the meetings, shall be filled by appointment of the President.

ARTICLE X.

QUORUM.

SECTION 1. Two-thirds of all the delegates shall constitute a quorum competent to alter or amend the Constitution.

SEC. 2. One-half of the delegates reported to the Secretary at any meeting shall constitute a quorum to transact any business except to alter or amend the Constitution.

ARTICLE XI.

POWERS AND DUTIES.

SECTION 1. The Society shall have full power, and it shall be a part of its duties, to adopt such measures as may be deemed most efficient for mutual improvement, and for exciting a spirit of emulation among the members

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of the profession; for facilitating the dissemination of useful knowledge; for promoting friendly intercourse among its members; for the advancement of medical science; and for securing the objects set forth in Article II of this Constitution.

SEC. 2. It shall have power to censure or expel any member convicted of violating its provisions, or who may be guilty of any act which may be considered derogatory to the honor of the medical profession; to hear and decide appeals coming from auxiliary societies; and enforce the observance of the code of ethics.

SEC. 3. It shall have power to raise money of its members, and those of the auxiliary societies, by a tax which shall not exceed three dollars, annually, upon each member.

SEC. 4. The Society shall hold at least one meeting annually, and more if deemed necessary for the promotion of its interests.

SEC. 5. It shall adopt a seal as an insignia of its corporate authority.

SEC. 6. The time and place of each succeeding meeting shall be determined by a vote of the Society.

ARTICLE XII.

EXPELLED MEMBERS.

Any member who may be expelled shall be debarred from the right of consultation, or any of the privileges of professional intercourse with any member of the Society or its auxiliaries; and shall forfeit all interest in its funds and property; but shall be held to the payment of any dues he may owe the Society at the time of his expulsion.

ARTICLE XIII.

FUNDS.

The funds of the Society shall be applied exclusively to the promotion of its objects, as set forth in Article II of this Constitution.

ARTICLE XIV.

CODE OF ETHICS.

This Society adopts, as a part of its regulations, the Code of Ethics of the American Medical Association.

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ARTICLE XV.

AMENDMENTS.

Every proposition for altering or amending the Constitution shall be made in writing; and if such alteration or amendment receive the unanimous vote of all the members present, it shall be adopted; but if objection be made, it shall lay over until the next annual meeting, when, if it receive two-thirds of the quorum for amending the Constitution, it shall be adopted.

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BY-LAWS.

SECTION 1.—ORDER OF BUSINESS.

1. The President, or in his absence, a Chairman elected for the purpose, shall call the meeting to order.
2. The Secretary shall call the roll.
3. Any business requiring early consideration may, by permission, be introduced.
4. Election of Officers.
5. Address of the President, if designed for the exclusive hearing of the Society.
6. Reading minutes of preceding meeting, or such portions as may be necessary for the information of the Society.
7. Reports of Officers.
8. Reports of Standing Committees.
9. Reports of Special Committees.
10. Reading of written communications on Medical Subjects.
11. Unfinished business.
12. Resolutions introducing new business, and instructions to Standing Committees.
13. Selection of time and place of next annual meeting.
14. Appointment of Standing Committees.
15. Miscellaneous business.
16. Correction and approval of minutes.
17. Adjournment.

STANDING RULES FOR CONDUCTING BUSINESS.

SECTION 2. The government of the deliberations of this body shall conform to parliamentary usage, unless otherwise determined by a vote of the meeting.

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DUTY OF PRIZE COMMITTEE.

SEC. 3. The Prize Committee shall consist of three members, and shall be appointed by the President at each annual meeting, as early as practicable. It shall serve from the time of its appointment until it reports to the next annual meeting. Whenever there is sufficient money in the Treasury for that purpose, the committee shall, before the close of the meeting at which it was appointed, announce that a prize of not less than fifty nor more than two hundred dollars in value will be given to the resident of this State who shall write the most acceptable essay upon the subject named by the Committee at the time. The Committee shall require every one writing for this prize to place his essay in the hands of the Committee, in the usual incognito manner, on or before the first day of May next succeeding. The Committee shall decide which essay, if any, is entitled to the prize, and shall report it, together with the key to the author's name, to the ensuing annual meeting, and the President shall, in the presence of the Society, break the seal and announce the name of the essayist, and shall thereupon issue to him an order upon the Treasurer for the prize.

REPORT OF PROCEEDINGS.

SEC. 4. The Secretary shall employ a short hand reporter, at an expense not exceeding ten dollars a day, to report and transcribe all the proceedings of the Society; and said report, after having been condensed by the Committee on Publication, shall be published in the Transactions. The Secretary shall prepare an abstract of the expense incurred in preparing said report, and said expense shall be assessed, per capita, upon the members, and collected in the same manner as the annual dues.

PUBLICATION OF LIST OF MEMBERS.

SEC. 5. The names of all the members who fill the requirements of the Constitution and By-Laws of this Society, and of all honorary members, shall annually be published with the Transactions.

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TRANSACTIONS

OF THE

INDIANA

STATE MEDICAL SOCIETY,

AT ITS

SEVENTEENTH ANNUAL SESSION,

HELD AT

INDIANAPOLIS, MAY 21 AND 22, 1867.

BOSTON MEDICAL
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PUBLISHED FOR THE SOCIETY.

INDIANAPOLIS:
DOUGLASS & CONNER, JOURNAL OFFICE, PRINTERS.
1867.

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TRANSACTIONS

OF THE

INDIANA STATE MEDICAL SOCIETY,

AT ITS

SEVENTEENTH ANNUAL SESSION.

INDIANAPOLIS, IND., May 21, 1867, 2 O'CLOCK, P. M.

The Indiana State Medical Society assembled in Eden's Hall, and was called to order by the President, Vierling Kersey, M. D., of Richmond.

The President said:

GENTLEMEN:—Perhaps it would be proper, on the present occasion, to recur to what should be regarded as a high honor shown me at our last annual meeting, by the Society, in selecting me as its President for the past year. I thank the Society.

J. A. Comingore moved that the Committee on Credentials be filled by the appointment of two members to fill the places of W. B. Fletcher and J. M. Kitchen, who were absent. Carried.

The Chair appointed H. G. Todd and William Lomax.

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W. R. Winton, from the Committee on Credentials, reported favorably on the credentials of the following Delegates, who paid their annual assessment:

NAME.	POST OFFICE.	SOCIETY.
Joseph Jones.....	Williamsport.....	Warren County Medical Society.
W. D. Lyons.....	Huntington.....	Huntington Co. Medical Society.
R. A. Curran.....	Huntington.....	Huntington Co. Medical Society.
J. W. Pugh.....	New Cumberland..	Grant County Medical Society.
C. E. Riggs...	Rigdon.....	Grant County Medical Society.
H. H. Beeson.....	Xenia..	Grant County Medical Society.
A. D. Kimball.....	Xenia..	Grant County Medical Society.
H. V. Passage.....	Peru.	Wabash County Medical Society.
J. R. Weist.....	Richmond.....	Wayne County Medical Society.
S. S. Boyd.....	Dublin	Wayne County Medical Society.
H. M. Minesinger...	Sulphur Springs...	Henry County Medical Society.
N. D. Clauser.....	Hartford City.....	Blackford Co. Medical Society.
C. S. Arthur.....	Pennville.....	Blackford Co. Medical Society.
Henry C. Kirby....	Granville	Blackford Co. Medical Society.
Rush Carley.....	New Salem.	Rush County Medical Society.
M. H. Bonnel.....	Lebanon	Boone County Medical Society.
T. H. Lane.....	Northfield.....	Boone County Medical Society.
G. W. H. Kemper..	Muncie	Delaware Co. Medical Society.
Robert Winton.....	Muncie	Delaware Co. Medical Society.
F. J. Vanvoorhis....	Stockwell	Tippecanoe Co. Medical Society.
W. Labarree.....	Clark's Hill.....	Tippecanoe Co. Medical Society.
S. B. Robbins.....	Lawrenceburg	Dearborn Co. Medical Society.
M. V. B. Newcomer..	Tipton.....	Indianapolis Acad. of Medicine.
R. F. Bence.....	Indianapolis	Indianapolis Acad. of Medicine.
Boswell Ward.....	Indianapolis.....	Indianapolis Acad. of Medicine.
G. N. Duzan.....	Zionsville	Indianapolis Acad. of Medicine.
G. V. Woolen.....	Indianapolis	Indianapolis Acad. of Medicine.
W. F. Boor	Newcastle.	Newcastle Medical Society.
R. Q. Wilson.....	Kokomo.....	Howard County Medical Society.
H. P. Anderson...	Monticello.....	White County Medical Society.
P. W. Payne.....	Franklin.....	Johnson County Medical Society.
J. T. Jones.....	Franklin.....	Johnson County Medical Society.
J. J. Saddler.....	Franklin.....	Johnson County Medical Society.

The following permanent members were present, and paid their annual assessment:

NAME AND RESIDENCE.	NAME AND RESIDENCE.
Corydon Richmond, Kokomo.	John Rea, Newcastle.
William Lomax, Marion.	Thomas B. Harvey, Indianapolis.
C. Lomax, Marion.	Wilson Lockhart, Indianapolis.
J. Shively, Marion.	S. W. Thompson, Evansville.

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NAME AND RESIDENCE.	NAME AND RESIDENCE.
L. Williams, Marion.	W. S. Haymond, Monticello.
L. Hess, Marion.	Vierling Kersey, Richmond.
H. D. Reasoner, New Cumberland.	S. H. Charlton, Seymour.
L. Corey, Warren.	R. H. Buck, Kokomo.
S. S. Horne, Jonesboro.	L. H. Kennedy, Belleville.
Henry Charles, Jonesboro.	H. C. Vincent, Guilford.
J. M. Bell, Dublin.	James K. Bigelow, Indianapolis.
A. M. Vickrey, Tipton.	L. Kern, Alto.
N. L. Wickersham, Anderson.	George W. Slack, Yorktown.
John Arnold, Rushville.	H. H. Moore, Indianapolis.
A. C. Dillon, Knightstown.	Jas. S. Athon, Indianapolis.
J. I. Rooker, Castleton.	Jas. H. Woodburn, Indianapolis.
W. A. Pugh, Rushville.	C. N. Blount, Tipton.
W. P. Waring, Richmond.	R. E. Haughton, Richmond.
William H. Wishard, Southport.	A. G. Preston, Greencastle.
Joseph A. Stillwell, Brownstown.	Wm. F. Harvey, Plainfield.
John T. Floyd, Noblesville.	H. G. Todd, Danville.
J. W. Martin, Lebanon.	J. W. F. Gerrish, Seymour.
D. B. Hall, Connersville.	A. H. Bryan, Indianapolis.
John Lewis, Ogdensville.	E. H. Crippen, Moscow.
Amos Chapman, Alquina.	John A. Comingore, Indianapolis.
R. Brand, Lebanon.	J. S. Bobbs, Indianapolis.
John Moffett, Rushville.	Theophilus Parvin, Indianapolis.
Dougan Clark, Richmond.	L. D. Waterman, Indianapolis.
James F. Hibberd, Richmond.	Thad. M. Stevens, Indianapolis.
W. R. Winton, Wabash.	A. G. Boynton, Elizabethtown.
J. R. Hinkle, Sullivan.	A. L. Underwood, St. Paul.
J. J. Thompson, Sullivan.	James Thompson, St. Paul.
George Sutton, Aurora.	N. P. Howard, Greenfield.
J. N. Parr, Zionsville.	Wesley Allen, West Union.
D. H. McDonald, Quincy.	

The following permanent members sent their annual assessment to the Secretary:

NAME AND RESIDENCE.	NAME AND RESIDENCE.
A. C. Irwin, Jalapa.	I. Mendenhall, Newcastle.
George Egbert, Xenia.	J. H. McIntyre, Richmond.
A. Henley, Fairmount.	R. R. Washburn, Waldron.
S. M. Linton, Columbus.	J. E. Lyons, Huntington.
A. J. McLeod, Burnsville.	D. S. Layman, Huntington.
H. Winton, North Manchester.	Wm. M. Reasoner, Sulphur Springs.

L. D. Waterman reported the probable absence of Horace Winton, Assistant Secretary, and moved the appointment of Green V. Woolen, Assistant Secretary *pro tem.* Carried.

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James F. Hibberd moved that a committee of five be appointed, to report to the Society on to-morrow afternoon the names of suitable persons for officers for the ensuing year. Carried.

The Chair appointed as follows: William Lomax, J. A. Comingore, William Waring, R. A. Curran, T. B. Harvey.

J. A. Comingore moved that the annual address of the President be postponed until 8 o'clock this evening. Carried.

On motion, the reading of the minutes was dispensed with.

L. D. Waterman, from the Committee on Publication, submitted the following report:

Your Committee on Publication have published since the last annual meeting:

500 copies Transactions	$238 00
300 Circulars	2 75
The postage on 360 copies Transactions, 4 cents each	14 40
Total	$255 15

Your Committee call attention to the fact that a paper on "Progress of Medical Science," by H. C. Cole, M. D., of Kokomo, Indiana, read before the Society at its last meeting, and referred to the Committee, is a literal copy of an address published in the Transactions of the Medical Society of the State of New York, 1864. The Committee recommend that proper steps be taken by the Society to express its censure of this plagiarism.

L. D. WATERMAN, *Chairman.*

The report was accepted.

W. R. Smith moved the appointment of a committee of three to examine into the charges alleged against H. C. Cole.

R. A. Curran moved to amend so as to place the matter in the hands of the Committee on Ethics, with instructions to that committee to report at the first session to-morrow.

The motion, as amended, carried.

The names of the Committee on Ethics were called by the Secretary, and Joel Pennington and F. S. C. Grayston were absent. The Chair filled the committee by the appointment of Dougan Clark and W. R. Winton.

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W. B. Lyons, Treasurer, submitted the following annual report:

TREASURER'S REPORT.

Received of Wm. F. Harvey, Secretary		$310 00
Received of J. H. Woodburn, Treasurer		104 29
Received of J. T. Strong, $3, and L. R. Johnson, $1		4 00
Received $2, each, from Drs. Iler, H. F. Barnes, W. B. Fletcher, J. M. Kitchen, G. W. Clippinger, J. I. Rooker, R. M. Wellman, J. M. Cochran, N. H. Canaday, J. Sloan, H. G. Todd, N. P. Howard, J. Moffett, A. C. Dillon, S. M. Linton, and R. E. Haughton		32 00
Total received to May 20, 1867		$450 29
Paid Douglass & Conner, printing Transactions	$238 00	
Paid W. & J. Braden, stationery	2 75	
Paid Downey & Brouse, circulars	2 75	
Paid L. D. Waterman, Secretary, postage on Transactions,	13 80	
Paid W. F. Harvey, late Secretary, expenses	7 76	
Paid Indianapolis Herald, advertising	75	
Paid L. D. Waterman, Secretary, postage and stationery,	3 47	
		$269 28
Cash on hand, May 20, 1867		$181 01

W. B. LYONS, *Treasurer.*

The report was referred to the Committee on Finance, who reported as follows:

The Committee on Finance respectfully report that they have examined the accounts of the Treasurer, W. B. Lyons, and find them correct.

They recommend that an assessment of two dollars be made upon each permanent member of this Society, and each member of every auxiliary society, for current expenses.

J. H. WOODBURN, *Chairman.*

The report was concurred in.

James F. Hibberd, from the Committee on Prize Essays, submitted the following report:

The Committee on Prize Essays report that they have received three Essays, but are of the opinion that no one of them is of that high merit required for the bestowal of the prize.

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They recommend that these Essays, with the accompanying sealed packets, be retained by the Secretary one year, and then burned without breaking the seals, unless sooner claimed and identified by their respective authors.

They recommend that the thanks of the Society be returned to the "Cincinnati Journal of Medicine," the "Lancet and Observer," of Cincinnati, and the "Chicago Medical Examiner," for gratuitously advertising the prize; and that the bill of the "Chicago Medical Journal" be paid.

JAMES F. HIBBERD, *Chairman.*
THEOPHILUS PARVIN.

On motion, the report was accepted.

R. A. Curran moved that an order be drawn upon the treasury for eight dollars, the amount of the bill presented by the "Medical Examiner" for publishing the call of the Committee for Prize Essays. Carried.

J. F. Hibberd moved that a new Committee on Prize Essays be now appointed. Carried.

The Chair appointed the following: J. S. Bobbs, Indianapolis; S. S. Boyd, Dublin; A. G. Preston, Greencastle.

H. V. Passage moved that the Secretary be instructed to inform the Secretaries of all the auxiliary societies throughout the State, that the State Medical Society requires from them a contribution of two dollars per capita of their entire membership. Carried.

J. F. Hibberd, from the Committee on Cholagogues, submitted a report on that subject — partly verbal and partly in writing. The unwritten portion of the report was, in substance, as follows:

MR. PRESIDENT AND GENTLEMEN:

Two years ago I presented to the Society a paper on The Liver, in which I attempted to give something of its development, histology, physiology, pathology and therapeutics. It was written pointedly for the purpose of exciting an interest in the Society that would entertain us, at least by calling out gentlemen who might entertain views different from those expressed in that paper. It had the desired effect; and so much of it as declared that the writer did not believe we had anything in the entire range of the Materia Medica that is properly a cholagogue, elicited a spirited and protracted debate; and one gentleman promised to present to the Society, at the last meeting, the facts going to show that, in his judgment, we had several such

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agents. A paper was accordingly presented at our last meeting, taking a much wider range than the subject of cholagogues properly warranted. It was a good paper, and brought forth a large amount of applause. I, at that time, made declarations similar to the views which I had incorporated into my paper, and then promised the Society that, while I could not then determine what my views of the paper before us were, nevertheless I would consider that paper carefully, and would come forward at the meeting of the Society this year, and give in full my views as to whether the views of the author of that paper were correct or otherwise.

I claimed, on that occasion, that this Society owed me a debt of gratitude; because, while the paper I had originally presented had but little merit in itself, it had, on the other hand, had the effect to excite debate, and also to call out an able paper in reply. I have come here on the present occasion with a paper on the same points that I presented two years ago, and which were so ably controverted at our meeting one year ago, and which I have stated over again to be identical with my present views. I should have been much pleased to have heard the paper re-read, in order that the Society might have had the opportunity of appreciating more fully the value of the gentleman's arguments in favor of the views which he has advanced.

It was understood, when Dr. Kersey's paper on cholagogues was read, that it was an attempt to give the scientific basis on which the views it presented rested; and because I thought that the views of the authors quoted, if not mistaken by the author of that paper, were, at least in many instances, presented in such a way that the ideas, as communicated to the hearers of that paper, were quite different from those intended to be conveyed by the authors quoted, it seems to me that a review of the case might be profitable to the Society. The paper opens with some general principles and comprehensive truths, with numerous glittering generalities with which I have no fault to find, but with which, on the contrary, I fully agree. But when it comes to the facts upon which the article professes to be based, they are presented in such a way that they are not entitled to that consideration which they at first seem to be.

I think he said that there are from twenty-five to thirty pounds of fluids presented to the liver of a man every twenty-four hours, from which to select the materials out of which to form the bile. There is no warrant for that assertion. Those matters pass to that portion of the liver that manufactures sugar exclusively. Hence, all that portion of his argument which rests upon that statement must fall to the ground.

Another point to which I will barely call attention, is the difference between the specific gravity of the contents of the portal and hepatic veins, and the different portions of serum contained in them. I have not been able to see that these facts, admitting them to be true as stated, have any bearing on the question at issue. If the specific gravity be differ-

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ent, and the amount of serum greater in one case than it is in the other, I do not see how that proves that the bile is derived from the portal blood.

A little further along is a declaration that the amount of fat contained in the portal blood, as compared with that of the hepatic vein, was a testimony that the bile was drawn from the portal blood, and from nowhere else. Even admitting the truth of the premises here laid down, I do not consider the conclusion as being at all legitimate; but the facts themselves are not well substantiated. Some authors speak thus of the matter, but later and more credible ones declare the opposite. Draper says there is more fat in the blood of the hepatic vein than there is in the portal blood, and infers from that fact that one of the duties of the liver is to make fat instead of separating it from the portal blood.

Another point was, that because it appears that the flow of bile is greater while the process of digestion is going on, therefore it must be that the bile is drawn from the substances being digested. But Prof. Lehmann says more than that: he states that the flow increases in quantity for no less than fifteen hours after the taking of food. This statement would wholly destroy the position that the bile is derived from the substances that are at the time undergoing the process of digestion. This is, I think, a mistake. Professor Dalton shows conclusively that the greatest amount of bile is secreted about one hour after the taking of food.

The report reads thus: "Many of the above facts, physiological and pathological, point directly to the portal blood as the source of bile. This idea is fully sustained by nearly all eminent microscopical anatomists." Now, I do not see how microscopical anatomists, as anatomists, could authoritatively make any statement as to where the bile comes from. Perhaps my opinion on this subject would be found to be worth more if the precise places where these eminent anatomical gentlemen have made that statement were pointed out. But the report further reads: "It is believed to have been demonstrated by Kiernan, Lereboullet, Beale, and recently by the well-directed and persevering labors of our sagacious countryman, Professor Salisbury. On the other hand, Morel teaches in his Manual of Histology, and Dr. Hibberd affirms in his paper on The Liver, that 'the hepatic artery alone must furnish the ingredients for the bile.'" I am glad to find that there is one intelligent physician in Indiana who has been able to discover anything sagacious in the productions of Professor Salisbury of Cleveland. His published papers, could we take them for truth, would be almost miracles. In one published a year or two ago he gave accounts of microscopic phenomena such as we could have seen only with such a wide field of vision, and such a magnifying power, as we cannot imagine in the present state of the manufacture of microscopes.

In reference to the statement attributed to me here, I must say that I never made it. So far from affirming Morel's statement, I simply quoted it and

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used these words: "This statement of Morel's lacks, as yet, the confirmation of other observations." This was equivalent to a positive statement that, in my opinion, it had not been proven, but would probably hereafter be substantiated. I think now that further investigation has elicited information that will place on a firm foundation the truth of the surmise contained in that sentence.

Whilst I accord to Professor Lehmann as much credit as any other gentleman, it must not be forgotten that the book from which Dr. Kersey has drawn all the information attributed, in his report, to Professor Lehmann, was written in 1841, more than a quarter of a century ago. And, if I understand the sentiments which he has attributed to Professor Lehmann, (and they constitute an overwhelming majority of all his references,) the sentiments here expressed are by no means identical with those of Professor Lehmann at the present time. That gentleman wrote this book in 1841. The second edition was published in 1850; and I presume it was from that edition that Dr. Kersey has drawn his facts. Professor Lehmann says in the preface that the knowledge of the subject on which he treats has advanced so much since the first edition came out, that he found it necessary to revise and alter the work in many respects, in order to bring it up to the existing standard. Now, if in the nine years that intervened between 1841 and 1850, the advance of science was such as to compel him to re-write his book, what should we naturally expect to be the case in the sixteen years that have elapsed since the issue of the last edition? We all know that within that period the advance of science, especially in the department of physiological chemistry, has been more rapid than in any other portion of the Christian era. What, then, is the value of a work on such subjects, written seventeen years ago, as a source of authority and a basis of investigation? It is true that all those facts asserted in that work, and not since contradicted by Professor Lehmann, or controverted by other writers, must be taken as still of good authority; but wherever you have two conflicting declarations from men of equal standing, but one made years ago, while the other is of recent date, science and wisdom dictate that we shall take that declaration which was made last. In the paper to which these remarks are merely introductory, I, too, have drawn from Professor Lehmann, but from another work, published more recently—his "Chemical Physiology." He there states that what the work contains is the substance of the former in a condensed and corrected form. In the original work he is very diffuse in his descriptions. The later work is the substance of the former adapted to the improved state of the science upon which he treats.

On page 38 of the printed Transactions of this Society for last year, Dr. Kersey says: "Valentin says the offensive odor of healthy fæces is much more dependent on decomposed constituents of bile than on the remains of food." This reference he follows up by a quotation from Professor Lehmann,

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which is as follows: "In jaundice, with occlusion of the biliary duct, the excrements are of a dirty, whitish-grey color and putrid odor." Now, in the first sentence we are made to understand that the offensive odor is dependent upon the presence of bile in the fæces, while in the second, it is found to exist where there is occlusion of the biliary duct and no bile at all in the fæces! I do not see the consistency of the two statements.

The next paragraph of the report reads as follows: "Wehsarg, who was extensively engaged in analytical observations on normal fæces, says the color varies with the food; on mixed diet they are of a yellowish-brown tint; on flesh diet they are much darker; and on milk diet quite yellow." How that extract could be regarded as showing that the color of fæces depends on the presence of bile I do not understand.

If any gentleman can see how the last two paragraphs on page 39, Transactions of 1866, establish the conclusions claimed, his comprehension is greater than mine.

Let us glance at another point or two in this paper. There is a statement here to the effect that "the effects of the suppression of the bile are generally considered to be too well known to require testimony; but inasmuch as this fact also is discredited in the paper on The Liver, I here cite the simple declaration of Professor Dalton on the subject." Now, so far from discrediting that fact, I expressly stated it in the paper on The Liver.

On page 43 of the Transactions for 1866 there is an account of experiments, made by Dr. Scott upon a dog, in relation to the formation and discharge of bile. He says: "The dog was in good health, *weighed eighteen pounds*, and was fed on milk, rice and boiled horse flesh." A simple mathematical calculation would be sufficient to show that if the dog weighed eighteen pounds, the dose of calomel given him was not equivalent to a dose of two drachms for a man of one hundred and sixty pounds weight, as it is stated to have been. As three grains (the smallest dose for the dog) is to eighteen pounds, (the dog's weight) so is thirty grains (the minimum dose for a man) to one hundred and eighty pounds, and not one hundred and sixty, according to the text. I regard it as clear that the calculation is incorrect.

Further on there is an attempt to establish the value of mercury as a cholagogue by comparing it to opium as an anodyne. I think it would be exceedingly difficult to see the parallel. I do not understand that there can properly be said to be any such thing as an anodyne where there is no pain to suppress.

In relation to Professor Dalton's declaration about the color of the contents of the small intestines, I have only to say that I have looked at the declaration of Professor Dalton upon that subject, and while I have no reason to doubt what he says—he was experimenting upon dogs—it occurs to me that there might be such a difference as to make us cautious in making the inference from them to man.

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J. F. Hibberd then read a paper on Bile, discussing some of the points presented in the former ones.

R. A. Curran moved that the Report on Cholagogues be accepted. Carried.

The Society adjourned to meet at eight o'clock this evening.

EVENING SESSION.

8 O'CLOCK, P. M.

The Society met pursuant to adjournment.

The President delivered his annual address.

W. Lomax moved that the President's address be referred to the Committee on Publication, with instructions to print it. Carried.

T. Parvin, having obtained leave to present a matter of importance to the Society, said:

MR. PRESIDENT:—It would be an interesting question to ask—How many physicians are there in the State of Indiana who do not take or read any medical journal? The number who do is small to what it ought to be. Probably not as many as every other man engaged in the practice of the profession takes any medical journal. Might not a great many of them be induced to take one if it was published in our own State? Certainly the result would be highly beneficial to the profession. Even the President, in his annual address, brought forward what I can easily construe to be a strong argument in favor of the establishment of such a periodical, when he said the result would be to stimulate men to write. Now I do not think there would be any damaging results that would follow if a large proportion of our medical men were thus stimulated; and I think I could pick out many men in our profession in this State, who can write both good English and good medicine, and who could be made useful to the profession in that way. I confess to you, gentlemen, that I have, in this matter, some state pride. I am proud of the State, and of the profession; and it is because I wish to see the profession of the State wield such an influence, and occupy such a position, as will reflect honor upon

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Indiana, that I am anxious that we should have an Indiana Medical Journal. The State of Indiana, with a population of probably two millions, destitute of a medical journal! Three thousand physicians in the State, having no medium by which their voices may address each other and the physicians of other parts of the country—no journal that shall reflect honor upon the State, win for itself a national reputation, and be one of the army of pioneers in the grand progress of Medicine! I want to see a medical journal permanently established, that shall reflect honor upon the State—a journal that will bind the members of the profession more firmly together, so that they shall be able to exercise a power that shall be felt upon the legislation of the State—that shall increase knowledge, elevate the professional tone, and benefit the whole community. A medical journal, properly conducted, can and will accomplish this. I have been led into this train of remark by a most unexpected assault upon it in the address of our President, whose objections were couched in those forcible sentences which he knows so well how to use.

Two years ago two gentlemen and myself established a medical journal. We set out with a determination to make it one of the best medical publications in the United States. Circumstances transpired which prevented our attaining to full success in our enterprise. In consequence of my determining to leave Cincinnati and return to Indiana, its success has been impeded to some extent. To bear my part in conducting it in Cincinnati, after my removal here, embarrassed me very greatly. I have had it in my mind ever since I returned that some day or other, that journal should be published in Indianapolis, and should be a journal for the State of Indiana.

I want to publish this journal at Indianapolis, commencing in July. The expense of publishing your Transactions amounts to between two and three hundred dollars for one hundred and ten pages. The journal is printed better, on better paper, with better type, better every way; and each number contains sixty-four pages. The expenses of publication are a little over two hundred dollars per month. The expenses for half a year would be something over twelve hundred dollars. Now suppose that sixty of the gentlemen who are in this room say to me to-night, "I will pledge myself to secure twelve subscribers for six months." We have already some three or four hundred subscribers in the State. If I can secure even as many as six hundred more, that will enable me to get along for the remainder of this year, at a loss of some three or four hundred dollars, which I will cheerfully bear; and next year it will pay for itself. I am not after money in this matter.

Suppose, then, that sixty of the professional men of Indiana come up and say, "Count on me for ten subscribers"—there is hardly a man here that can not do that much with proper industry—the journal would be estab-

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lished at Indianapolis. But suppose that any gentleman should fail to make up the number of subscriptions for which he pledges himself. Is there a gentleman here who cannot afford to pay that much for the purpose of establishing such a publication in our State? Surely not.

And I will say, further, that if you think that other gentlemen had better be associated with me in this matter, you can appoint them, and I will work with them; or, if you think that some other gentleman can do the work better than I can, just so soon as I am released from present obligations, I will resign and let it go into his hands. I am ready to throw aside all personal ambition, and everything of that kind. Individuals are nothing in comparison with an enterprise like this. Men are nothing in comparison with the progress of the Science of Medicine. Let me urge you to take this matter right home to your hearts and minds, and not only to feel the importance of the advancement of Medical Science—not only to think about it, but to act at once. Go to work, and do something this very night to help it along. Let us have this thing fixed right now, and when it is done I shall feel, for one, that it is the longest step toward progress that the profession in Indiana has ever taken.

V. Kersey said:

Mr. President:—I did not see, at first, how it was possible that any good should come from my annual address. I now see that good has come of it; for we have had a most stirring, eloquent, patriotic, and science-loving speech, that never would have been made if it had not been for my address. I believe Dr. Parvin will fully endorse my veracity, when I say that I had not the most remote intimation that any person had any idea of establishing a medical journal in the State at the time I wrote that address. I spoke of the matter abstractly, without any idea that it would be applied to any incipient enterprise of the kind. But if I had been called upon to find a man that would make such an enterprise win, I think in all probability I should have selected Dr. Parvin—after hearing his speech, I *know* I should. If anything I have said has had any effect upon the mind of any one, I trust it will be forgotten at once, and that all gentlemen here will cordially second Dr. Parvin in his enterprise.

T. B. Harvey said:

Mr. President:—I desire to express my gratification with your annual address. But it has some sharp corners that require rounding. I allude to the remarks it contains on the subject of a medical journal and medical college. I recollect to have read productions from yourself, written when you were only a country doctor. In the State of Indiana there are men whose experience, if expressed through the medium of a medical journal, would be highly valuable to the profession.

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For fear that the address of our President may be the means of preventing that very feeling throughout the State, which should be gotten up through this association, I think the remarks that have been made here in allusion to the subject of a medical journal should go to the profession with the address. The President's explanation should go with the rest. It is true the country is flooded with medical journals, but they are all of them foreign to this State. I think I would be safe in saying that Indiana is at this time almost the only State that has no medical journal. Why is it so? There are men in this State who have valuable experience. True we have not here opportunities favorable to those fine pathological examinations that some other States have; but, on the other hand, we have experience suitable for the location in which we practice. Hence we all need such information as would be contained in the pages of a journal published in our own State.

The medical men of Indiana were, to a certain extent, disgraced during the rebellion. While there were surgeons from Indiana in the army who would have reflected honor upon any State in the Union, there were also men in that position who would have been a disgrace to any locality. Of course the remark I am making cannot be appropriated by any gentleman here who may have been in the army; for we all know that the class of men to whom it applies do not appear in associations of this kind.

What shall we do to redeem the character of the profession in this State, so far as it has suffered from this cause? Let us organize and work. I know of no better means of accomplishing the end in view than to establish on a firm basis this medical journal. It is a good one. If it is thought proper to do so, let this Association take an interest in it by way of controlling it; at all events, let us have a medical journal.

The question of a medical college is one of great importance. If this address should come before the people of the State without explanation, it would seem to the public to have the endorsement of the Society. The inference would be that we felt ourselves incapable of sustaining a medical college, and did not desire any such thing in this State. There are men throughout the State looking forward to the establishment of such a medical college, who would regard the movement in an unfavorable light after such an expression as that from this Society. In all probability it would result in the defeat of any legislation that might be sought in relation to the matter.

J. I. Rooker said:

Mr. President:—I think, if you could look down on all portions of the State of Indiana, you would see the State as one large hospital, from which a vast amount of practical information could be derived, if it was gathered within the compass of a medical journal. Many of the most valuable articles in the various medical journals are written by men in the State of Indiana.

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We have medical men enough in this State to sustain such a journal, as well as talent, experience, and science enough to make it valuable. I hope to see it well sustained.

Wilson Lockhart said:

MR. PRESIDENT:—Some years ago I was traveling along the route where the Hannibal & St. Joseph Railroad now lies, and in talking with a gentleman of the legal profession, in reference to a railroad through that section of country, he said, "I do not expect it will be built in my day." Two years from that time the railroad was in running order. We need not wait for our children to establish a medical college or a medical journal in the city of Indianapolis. We can do it ourselves; we ought to do it; we can do it, if we will, within the next year or two. We should take hold of the matter at once, for our credit if nothing else. I will furnish Dr. Parvin ten subscribers.

W. R. Winton said:

MR. PRESIDENT:—Dr. Parvin's proposition is not new to a great many members of the profession in the northern part of the State. It has been talked of there. It is a reproach to the profession to have no organ, and to have so many hundreds of young men go out of the State every year to be taught in medical schools. Many men would take a medical journal if it were published here, who would not take one published anywhere else. Journals from the Eastern States do not always show us how we can best manage diseases in the State of Indiana. A physician coming here from New York, however well educated he may be, must have a personal experience here before he can treat diseases with full confidence. What we most need in a medical journal is the experience and observation of men who are familiar with the diseases, and their peculiarities, of our own State. I will take ten copies.

W. Lomax said:

MR. PRESIDENT:—I believe that the sentiments of the President on this subject are precisely my own. I know that he as ardently desires the welfare of the profession in Indiana as any other man. His apparent opposition to a medical journal was simply opposition to an inefficient journal. The same remark will apply to his sentiments as to a medical college. He was not objecting to the establishment of a first class journal or a first class college. There was, however, in connection with the remarks he made upon that subject, one sentiment—ignoring all State lines—in which I can not concur. I was in the army as a Surgeon from Indiana, and was often mortified at hearing, from the Surgeons from other States, that Indiana was the only State that had ignored qualification as a condition of admission to a

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surgeoncy. I heard them say that some of the men who had, in consequence, found their way into the army were a disgrace to the State and to the profession. I took it upon myself to call a meeting of the Surgeons in our Division who were from Indiana. We held some consultation, and determined that we would never cease our efforts to elevate the standard of the profession in our State, until it stood as high as that of any other State in the Union. We concluded that if three thousand physicians of respectability could operate as one man for the advancement of the profession, they could accomplish all that human power could accomplish to elevate and improve our science. United, we can wield a powerful influence in behalf of any measure for the benefit of the profession. Three thousand physicians, enjoying the confidence of the community in every part of the State, can mould public opinion in any necessary direction, and secure any legislative enactment that may be necessary for the advancement of the interests of the profession. We can secure such legislation as will effectually protect the community from imposition. These measures should be presented to the profession through the medium of the State Medical Society; and, depend upon it, when properly and thoroughly organized, whatever reasonable measures are so presented, will be carried out. Unite the profession in this matter, and we can not only raise a fund that will publish all reports of value, but sustain a journal and a college of high character, build up the profession, and make the science and practice in Indiana fully equal to that in any other State in the Union. I am in favor of building up a college, a journal, and a profession that will compare favorably with any other in the United States. And we can do it. The talent is here: there are young men in Indiana who will be ornaments to the profession. I am decidedly in favor of this enterprise.

J. A. Comingore said:

Mr. President:—Much as I admire the high character of the address you have presented to the Society, I must take exceptions to the sentiments it contains in regard to a medical college and a medical journal. The address warmly encourages the formation of county and local societies for the purpose of mutual improvement and scientific investigation: that is right and proper. But I think the President should, for the same reasons, and with the same end in view, have encouraged, instead of discouraged, the establishment of a medical journal, and eventually of a medical college. It is true that the profession always, here as well as everywhere else, needs some incentive to work properly and effectually. There is nothing that would bring the talent of the State out more than a medical journal. I think there is talent enough in Indiana to carry on a journal of the very best character. We intend to transfer to our own State a journal already established—one that has attained considerable character in the profession—

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and make it a journal of the State of Indiana; and I think, without hesitation, that it can be carried on successfully. This will lead, finally, to the establishment of a college for the purpose of advancing the cause of medical education in our State. Now, in looking over college statistics, I find that Indiana sends a great many students every year to colleges outside of her borders; and it is certainly humiliating to the profession in Indiana. I believe that, in the end, there will be established primary schools, intended to take students and prepare them to enter college. From the proceedings of the recent Convention at Cincinnati, we learn that something of the kind is desired. This is a move in that direction. The establishment of a State journal is the first step. I believe, from observation, that there is as much active and available talent in the profession in Indiana, as in the same number of men in any other State, and that it will be brought out by means of a medical journal more certainly and effectively than in any other way. It will be the starting point of a career of improvement that will end in something greater.

James F. Hibberd said:

Mr. President:—As a test, I move the adoption of the following resolution:

Resolved, That it is the sense of this Society that it is desirable to establish a medical journal in the State of Indiana.

J. S. Bobbs said:

Mr. President:—I wish to meet some points that were raised in the discussion. One was by my friend Dr. Lomax, in reference to the standing of the profession of our State in the army. It is true that in many localities the profession in Indiana did not tower very highly. But not in the slightest degree did that turn upon the conduct of the profession in Indiana, but upon that of the politicians. When the Legislature was in session, early in the war, a committee from that body requested me to draw up a law for the examination of applicants for surgeoncies. They said they were not going to send their boys into the field without knowing what kind of physicians were with them. I told them I would furnish them with the draught. They said they would see that the law passed. I furnished them what I thought was adapted to the purpose, and they expressed their satisfaction with it in every particular. They read it in the Legislature. It was very well received, and passed along smoothly until it came to voting, and then, just as I expected, it was voted down. The Legislature of the State of Indiana refused to require any qualification of a man applying for the position of a Surgeon in the army! The reason was that there were politicians in that body who wanted office — there were men who wanted to be Surgeons in the army who had not practiced scientific medicine for ten years. They

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wanted Surgeoncies, and could get them by defeating that bill. In the army no man discharged the duties of a Surgeon more ably than gentlemen whom I personally knew from Indiana. I found a great diversity of ability in the service. There was great complaint of the incompetency of Surgeons. I had quite a number of Indiana Surgeons, as well as Surgeons from other States, among those gentlemen over whom I had supervision. In order to be perfectly impartial, I instituted a board of examination, and invited every officer and every private, who had any grievance to allege against any medical officer, to bring it before that board. An Indiana Surgeon never came before that board while I was in the service, while Surgeons from other States did. The fault, where there was any fault, was not in the medical profession of Indiana, but in the faulty manner in which our men were sent to the field. Some men, to my certain knowledge, went into the army from Indiana, as Surgeons, who never could have got an appointment from any intelligent board of commissioners.

In reference to the establishment of a medical journal and a medical college, and the paper of the President relating to that subject, some gentlemen are taking exceptions to the remarks therein contained. I am sorry to differ with them. I do not understand the President as objecting to a medical journal or a medical college of respectability; but I understand him to insist that if we do anything of the kind, we do it right. Until we are able to take the standard of the profession in Indiana, and hold it up for the world to look at, we should not undertake such an enterprise. If we institute anything of that kind, he wants it to be such that we will not have to look at it and blush. And he is right in that. The establishment of a college and a journal, of the high standard of the President, would benefit the profession without a doubt. There is talent enough in Indiana to do it; but the difficulty is in combining that talent to sustain those two institutions. Bring the talent of Indiana to the support of a medical school or journal, and it has no favors to ask, for it will compel success.

S. S. Boyd said:

Mr. President:—I am satisfied that if we will establish such a journal as we ought, in Indiana, every medical man here will be in favor of it. If, as has been the case in other places, the enterprise has to be abandoned, it would be far better not to publish at all. I hope that we will succeed in establishing such a journal as will silence all cavil.

There were many surgeons from Indiana who did not stand as high as they should have stood; but other States sent surgeons who were equally low in the scale of intellect and professional attainments. Dr. Bobbs has alluded to the main cause that placed these incompetent men in the army. Other States had the qualifications of candidates examined into, while we

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had not. To-night is the first time I have ever heard that the Indiana surgeons in the army, stood below those of any other State.

V. Kersey said:

MR. PRESIDENT:—I am anxious to say a few words upon Dr. Hibberd's resolution, and I believe it will be necessary for me to refer to my annual address. I think I must have so written as to convey to some gentlemen ideas that never were in my head. I thought I was giving, by my address, a practical illustration of the poor qualifications of Indiana "country doctors" in the way of authorship! Dr. Hibberd says that those who do not vote in favor of his resolution will be understood to vote against the establishment of a medical journal in the State of Indiana. If I have conveyed the idea, in anything I have said or written, that I am opposed to good medical colleges or medical journals, located anywhere or published anywhere, I have been misunderstood. It is the quality of the article, and not its locality, that will give it currency with me. If I was not before clear upon this point I trust I now am. I would rejoice to see as good a college established at Indianapolis, this year, and as good a medical journal, as there is in the world. It would give me more pleasure to see them both established here than anywhere else.

James F. Hibberd said:

MR. PRESIDENT:—I understand that the Cincinnati Journal of Medicine, which every gentleman who has been a subscriber knows to be one of the best publications in the West, is intended, by Dr. Parvin, to be transferred to Indianapolis. The Cincinnati editors want to abandon it. For Dr. Parvin to conduct it there, and reside here, is exceedingly inconvenient. I understand him to say that it will cost some twelve or thirteen hundred dollars to carry on the journal from July to the end of the current year; but if it can be kept up that long, it will sustain itself for the future. He says that he is willing to bear more than one-fourth of that expense, and it is surely as much as ought to be required of him. He wants the medical profession in Indiana to pay the other three-fourths, which would be only about 33⅓ cents apiece. We should come forward, with willing hearts and full hands, and give it the needed support for that period of time. And I do not doubt that if we do so, we will find, before the end of that time, that it is one of the best instrumentalities we have ever had, and one that we could not do without. Can we raise the nine hundred dollars? I am willing to pay for twenty copies.

The resolution of Dr. Hibberd was unanimously adopted.

J. F. Hibberd moved that the Society adjourn to meet at half-past eight o'clock to-morrow morning. Carried.

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SECOND DAY.

MAY 22, 8 O'CLOCK A. M.

The Society met pursuant to adjournment. The President in the chair.

John Moffett announced that Prof. John A. Murphy, of Cincinnati, was present, and moved that he be invited to a seat with the Society, and to take part in its deliberations and discussions. Carried.

R. A. Curran gave notice that at a proper time he would introduce measures looking to the establishment of a medical college in the State of Indiana; and called a meeting of the friends of that movement for half past one o'clock this afternoon.

T. Parvin reported that nineteen members of the Society present had guarantied subscriptions for one hundred and seventy copies of the Cincinnati Journal of Medicine, leaving four hundred and thirty copies to be taken in order to secure the establishment of that journal in Indianapolis.

W. R. Winton submitted the following report from the Committee on Ethics:

The Committee on Ethics, to whom was referred the case of H. C. Cole, M. D., report that they have examined the manuscript of his paper on The Progress of Medical Science, as read before this Society at its last meeting. After a careful comparison of the same with a printed address delivered before the Medical Society of the State of New York, by Daniel P. Bissell, M. D., President, of Utica, February 3, 1864, they find it to be a literal copy of said address, with here and there an omission or alteration of a word or part

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of a sentence! and therefore find him guilty of an inexcusable plagiarism, and insult to this Society, for which they recommend the following:

Resolved, That H. C. Cole, M. D., is hereby expelled from this Society; and that the Secretary inform the Secretary of the American Medical Association of his expulsion.

The report was, on motion, accepted.

James F. Hibberd moved to adopt the resolution reported by the Committee.

The resolution expelling Dr. Cole was then unanimously adopted by a rising vote.

W. R. Winton proposed the following addition to the third article of the Constitution. Add the words:

All Delegates or Representatives, having served one year as such, shall be thereafter permanent members.

The Chair announced that the amendment would lie upon the table until the next annual meeting, in accordance with the provisions of the Constitution.

James F. Hibberd proposed an addition to the By-Laws, as follows:

SEC. 6. Where a member of this Society has been expelled from a local medical society, and the fact is properly certified to the Secretary of this Society, his name shall be stricken from the roll of this Society: *Provided*, That such expelled member shall have the right of appeal to this Society from the judgment of the local society.

The proposition was adopted.

James F. Hibberd moved that the next annual meeting of the Society be held at the city of Indianapolis. Carried.

J. A. Comingore, from the Committee on Arrangements, reported as follows:

The Committee made efforts to secure reduced fare on the several railroads, for the members in attendance upon this meeting, but in each instance without success.

The Committee secured the hall now in use, at an expense of five dollars per day, and five dollars per night. The Committee recommend that an order be drawn on the treasury for the amount.

On motion of D. Clark, the report was concurred in, and an order authorized to be drawn for the hall rent.

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On motion, Dr. Hibberd's report on Cholagogues was taken from the table for discussion.

V. Kersey discussed the report as follows (Dr. Woodburn in the chair):

Mr. President:—I desire to make only a few remarks upon this subject, touching the report. The ground traveled over by Dr. Hibberd on yesterday was so broad that had I been permitted to follow instantly, I have no hesitation in saying that I could not have reviewed it without being exceedingly restricted and embarrassed. I had no opportunity to take notes of Dr. Hibberd's extemporaneous remarks, and must now rely upon the slender resources of a very poor memory, for anything that I shall say in reply. I would not be disposed to discuss, for the third time, a question like this, were there nothing more involved than what might be considered by some as a mere personal controversy. For my own part, I take no such view of this question, neither am I disposed to attribute such a view to Dr. Hibberd. I take it for granted that whatever he says and does here, is said and done with the same motive that influences me—that of reaching the ends of truth without regard to personal considerations. Dr. Hibberd and myself are in the habit of meeting once a week, and have been accustomed so to do for five years, for the purpose of talking on the subject of medicine. I think that he and I fully understand each other. Perhaps sometimes there may be a little acerbity, or some other sour thing, between us for a moment; but in any discussion of this kind I lay aside every consideration of that kind. And I wish to attribute to him but one motive, as I have but one in all I say and do here.

Dr. Hibberd, in his preliminary remarks, gave you some insight into his opinion in regard to the merit of Prof. Lehmann's works and their order of publication. He referred you to two editions of Prof. Lehmann's Physiological Chemistry, and also to another published by the same author a little after the publication of the second edition, namely, his Chemical Physiology. In connection with his remarks upon these works, Dr. Hibberd said that I was in the habit of going back to old, and comparatively obsolete, books for my facts. I had quoted, he said, from the work on Physiological Chemistry exclusively, and not from the Chemical Physiology of Dr. Lehmann. And so did he, when he undertook to refute the positions I assumed, in his remarks on yesterday. He, too, quoted the old work, and not that on the subject of Chemical Physiology. I do not think he will deny that such is the fact. (Dr. Hibberd interrupted the speaker with a denial of the correctness of the statement.)

Dr. Hibberd talked yesterday as though he thought I had never seen the later of Dr. Lehmann's two works. I am quite as familiar with that work as I am with the other. It was the suitableness of the old work to the purpose

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that led me to quote from it. I trust that I have not quoted, in my whole article, a sentiment that is disavowed in that eminent chemist's more recent work. At least, if I have, it was not shown to me on yesterday.

Dr. Hibberd alluded to a little dog that I had mentioned, and the Doctor alluded to it very courteously, I must admit. I have authority for saying to you that the dog weighed about eighteen pounds. I will promise to satisfy him fully on that point. (Dr. Hibberd subsequently acknowledged that the weight is given at eighteen pounds and six ounces, on page 268, Trans. N. Y. State Med. Soc., 1864.) I suppose that there is, in fact, in the figures that I presented, an error of two grains! making it twenty-eight grains, instead of half a drachm, as the smallest dose for a man weighing one hundred and sixty pounds.

Dr. Hibberd quoted, quite largely, articles, statements, analyses, propositions and chemical results that I had myself carefully looked over in making up my report on Cholagogues. It is true I did not report the precise documents that he did, and for what I consider a very good reason; for it seems to me that when we are searching for a positive fact we do not care for merely negative evidence. When I want proof of the fact that a man has found bile in the solid, healthy excrements of a human being, I go to the analysis where the article is found, and not the analyses of half a dozen other cases where men sought for it and did not find it. That constitutes the difference between my documents and those of Dr. Hibberd. I presented you with the evidence that in a certain case a certain thing was found, whilst he attempts to controvert my evidence by showing that in certain other cases that thing was not found.

That Prof. Lehmann is a competent chemist; that he tells all these gentlemen who failed to find bile just why they did not find it; the Doctor ought to have been able to discover, even from my article. He tells these gentlemen just why they failed to find with their tests the appearances characteristic of taurocholic acid. He says that the bile undergoes decomposition in passing down the intestines, and that while the bile itself may not be found in the solid, healthy human fæces, yet a greater or less number of the elements into which it is dissevered and elaborated, during its passage through the alimentary canal, may always be found there in every case where the liver itself is not locked up and its secretions absolutely kept out of the canal. That is the only case in which the elements of bile are not to be found in the excretions. Gentlemen here will not expect me to reiterate the statements of Prof. Lehmann, and the views advanced year before last at the meeting of this Society, that I repeated last year. I do hold that bile is an entity and may be traced by a good chemist, one who knows a thing after it has changed its form a little, not only throughout the entire length of the alimentary canal, but into the excrements, and for a long time after that. But I do not hold that it is the only thing that deserves attention. I am not going to consume

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the valuable time of the Society by reiterations of opinions that are of no value whatever as coming from me, and that derive their only claim to the consideration of the gentlemen belonging to this Society, from their emanating from that high authority from which I claimed to have derived them without curtailment, addition or misrepresentation.

But suppose that we should all set ourselves up as chemists, and should all try to make an analysis of the same articles; it seems to me altogether possible that some one or other of the gentlemen here would fail. But even if they all succeeded, after a fashion, they might not all report precisely the same result. And if, in that case, a question arose as to whether or not there was present a given substance, would the fact that half a dozen of the analyzers, or a greater number, failed to find it, be taken as evidence going to controvert the analysis of competent persons who could state positively that they did find that element there? Certainly not. I have presented the testimony of chemists of the very first standing, who assert positively a certain fact, viz: that they found a certain thing in the solid, healthy excrements of a man. I also presented the testimony of a chemist and physiologist, who was pronounced by prominent medical men to be "one of the brightest men that ever lived in New York," and the man who had written "the best physiological treatise America has ever produced;" and that man tells us that he took the gall-bladder of a large dog, just as it came from the living animal, full of bile, and after subjecting it to every species of chemical analysis that chemistry furnishes—Pettenkofer's test and all—he utterly failed to find a trace of the presence of bile!

Dr. Hibberd quotes a statement contained in my paper of last year, upon the authority of Valentin, to the effect that the presence of bile in the fæces is shown by their offensive odor, the peculiar odor of healthy fæces being far more dependent upon the decomposed remains of bile than upon the remains of food. This statement, Dr. Hibberd says, is fully refuted by the quotation which follows it, in which Prof. Lehmann declares that "In jaundice, with occlusion of the biliary duct, the excrements are of a dirty, whitish-grey color and putrid odor." I confess my inability to see any contradiction here. There are two different things here spoken of. The first allusion is to the peculiar odor of healthy fæces; the second is to a putrid odor observed in the fæces, when the biliary duct is shut up. In the latter case there is no bile in the fæces, of course, and there is a "putrid" odor; in the former there is not the same "putrid" odor, but the peculiar odor of healthy fæces, and, according to Valentin, it is occasioned principally by the fact that there are in the fæces the decomposed constituents of bile. Instead of an inconsistency existing between these two statements, one of them is the exact complement of the other.

Dr. Hibberd quoted another set of my quotations on yesterday, in which

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he found, as in other cases, a sad irrelevancy between my premises and conclusions. Those quotations were as follows:

1. "The mean specific gravity of normal human blood is 1.055; that of its fresh serum, 1.028. The specific gravity of fresh human bile is about that of urine, ranging from 1.010 to 1.020."—*Schmidt.*

2. "One hundred parts of portal blood contain thirty-four parts of serum, while the same quantity of blood from the hepatic vein contains only fifteen parts of serum."—*Lehmann.*

I know it is sometimes a little difficult to see the connection between facts and what those facts should teach. Let me try to make myself understood. The portal vessels, containing the portal blood, and the hepatic vessels, containing the hepatic venous blood, are united by capillaries perhaps from an eighth to a quarter of an inch in length. There is no other vascular connection between them. Right where these capillaries cross, between the two sets of vessels, we have the acini of the liver, surrounded by a plexus of capillaries. Now, the portal vessels have thirty-four per cent. of serum in the blood; just after the blood has passed through the capillaries and around the acini of the liver, we have but fifteen per cent. of serum in the blood; showing a difference of no less than nineteen per cent. in the quantity of that liquid between those two points, but a small fraction of an inch apart. What becomes of the nineteen per cent. of serum? Is it evaporated? It is supposed that the liver does generate some heat, but I fancy no gentleman will claim that that quantity of liquid could be evaporated under such circumstances. Is it converted into sugar? The production of sugar is the only function claimed to be exercised there, besides the production of bile. Now, the bile that is elaborated in that little organ over which the capillary vessels form a beautiful plexus, has a specific gravity sometimes as much as eighteen per cent. below that of serum, and the specific gravity of the serum on one side is sometimes nineteen per cent. lower than it is on the other.

Dr. Hibberd replied to Dr. Kersey as follows:

Mr. President:—It is a thing unaccountable to me that, if Dr. Kersey was so familiar with the latest and most complete of Prof. Lehmann's works, he should have quoted so extensively from the old work, which the rapid advance of science has now rendered comparatively worthless. And it seems to me no less astonishing that he should have uniformly marked his quotations simply by the name of "Lehmann," if they were taken from the Chemical Physiology, the later of his works. I do not think these sentiments, which he has thus quoted as Prof. Lehmann's, are, even generally, from his last book. I believe them to be in "Lehmann" just as Dr. Kersey has presented them, but in most instances to have been drawn from the Chemical Physiology. It is strange that a gentleman seeking for a modern interpretation of facts and principles of science, should not betake himself to the very latest and best

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sources within reach. After I had read Prof. Lehmann's first publication I laid it down, and found no further use for it while preparing my report on Cholagogues; and if it should turn out, upon examination, that anything I have used is traceable to that work, it will be because the author has incorporated the same thoughts and the same language into his later work.

Dr. Kersey went into an elaborate argument for the purpose of showing that the derivation of the bile from the portal blood had been demonstrated as clearly and distinctly, to every properly constituted mind, as any problem in geometry. I suppose that to be conclusive, and that my not seeing the force of his reasoning is attributable to my own inability to comprehend those processes. But if I was at all successful in what I attempted to show on yesterday, then we must take it as a fact that the portal blood, when it goes through the capillaries, does not surround nor come in contact with those cells which produce the bile, but those cells, and those only, which make sugar. The evidence upon this subject is sustained by going, for analogies, to comparative anatomy, where we find not only animals having livers dual as to their functions, but also animals having two livers, entirely distinct and separate. In the latter we find a duct designed to carry the secretions of the liver into the alimentary canal, and supplied with blood from the arterial system alone.

But I do not desire to discuss this question further at present, and submit my paper with its views, its facts, and its authorities, to the society.

J. A. Comingore said:

Mr. President:—I have listened with interest to the reading of these papers, last year and this year, and have been highly entertained by both, as well as by the discussion that has followed. It is to be admitted that the authorities that both gentlemen present here, on which they found conclusions, are creditable, and the only question now is: which is correct? They have doubtless presented fairly all the facts going to sustain the views of the various authors. The question turns upon which is the most rational conclusion. For several years I have had this matter under consideration, and so, probably, have others, and it rests with each of us to draw his own conclusions as to which is the most credible.

I have come to the conclusion that cholagogues are not nearly as efficient as they were supposed to be twenty or twenty-five years ago. Whether or not the liver secretes a great amount of bile; and in what manner; and what influence the bile has in digestion; are questions of considerable practical importance; but the question as to what has most effect upon the secretion of bile is of more importance still. Calomel, and the various preparations of mercury, have long been regarded as the sheet-anchors of the profession in exciting the action of the liver. I have not, for several years, regarded mercury as exerting any peculiar influence upon the liver. It is

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my belief that where what are called cholagogues do any good, it is from their peculiar action on the alimentary canal; and by increasing its action, as evacuants, they exert an indirect influence upon the liver, relieving the portal congestion. Experience proves that what is termed "hepatic derangement" is relieved as readily with other remedies as with the specific cholagogues. Hence we come to the conclusion that cholagogues are practically of no great importance to us in the practice of medicine. It is ascertained from experience that the liver may be as readily relieved of functional derangement by any purgative, as by the reputed cholagogues. I am glad this conviction prevails. It shows progress; and since these conclusions have been adopted there has been less harm done than formerly by mercury. It is a fact that in the intermittent fevers, commonly called bilious, that heretofore have been regarded as requiring mercury, that agent is now no longer regarded as essential. It is ascertained that fevers will now recover as readily without the use of reputed cholagogues as with them.

R. E. Haughton said:

Mr. President:—I have followed these gentlemen carefully in the papers read; and while, during the last two years, they have been presenting us with the facts upon the question, I have been pursuing investigations into the effects of cholagogues. How much better it would have been for us, and for the Science of Medicine, had these gentlemen presented the facts themselves, instead of referring us to Dalton, Flint, Bidder, Schmidt, Lehmann and others, who have already given us in full their views and experiments. Medical men should cease to depend on others, and pursue investigations for themselves. If they have not the means of experimental investigation they should procure them, and be prepared to give us the results of their own investigations; which are needed now, and when we get them the profession will make a more rapid advance. I regret to see gentlemen willing to spend two or three years in passing over ground that has been traveled over before.

It has been attempted, on the one hand, to show that there is no evidence that chologogues produce any effect upon the secretion of bile—upon the action of the liver. What does experience say? Now, as the gentlemen have not given us any new facts from their own experience, we are compelled to fall back upon the experience of those who have. So far as the reference of these gentlemen to the authorities are concerned, it is my impression that the weight of evidence is that chologogues do actually produce the effects attributed to them. I am ready to admit that, in the history of the profession, cholagogues have done a vast amount of mischief. I believe it is high time for the profession to seek and apply a remedy for this, by declaring that we do not resort as pertinaciously to the use of cholagogues in the treatment of diseases as has formerly been done. I am willing to

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concede that my opinion has changed in regard to the value of cholagogues as remedies, and to say that I do not, in my practice, resort to them as often as I formerly did. It is a rare thing with me, at present, to prescribe calomel as a cholagogue. Yet I occasionally find cases of intermittent fever, and other diseases, attended with functional derangement of the liver where such an agent is of great value. I have found cases that I could not manage without the use of some one of the medicines known to us under the name of cholagogues, and I have found their use beneficial to the patient as a general rule, though perhaps not invariably.

John Moffett said:

Mr. President:—The members of the medical profession are naturally divisible into two classes. The great body of us are simply practitioners. The smaller class is composed of those men who positively increase our knowledge. The latter are necessarily few. It takes men who are uncommonly energetic and indefatigable, to come within that class. They are men who are ambitious to have it said of them, in the future, that they added to the stock of our positive knowledge. That is the kind of work that we most need. We want men who have leisure, talent and inclination, to look deep into those subjects that are beyond the reach of the ordinary practitioner; and place the results within the reach of the mass of the profession. The duties required, from day to day, of the practitioner, are such as to render it impracticable for him to pursue such investigations and experiments as are required by these abstruse and difficult questions. We, as a Society, are highly indebted to the gentlemen who have been engaged in this discussion, for the highly interesting and instructive manner in which this important subject has been placed before us.

What benefit have we, as practitioners of the healing art, derived from the presentation? How much better prepared are we to approach the bedside of the sick man than we were before? If we are to take the sum total of the facts adduced in the report of Dr. Hibberd as evidence conclusive of the correctness of the views therein expressed, what man of us will be able to tell, in his future practice, whether or not the liver is deranged in its functions—whether it is acting, or not acting at all?

The views contained in the remarks of Dr. Kersey, if we follow them, enable us still to retain the usual signs that practitioners have been accustomed to recognize as evidences that the liver is inactive. If we hold to the latter opinion we have still that guide—while by the former it is ignored. How are we going to determine this when called upon to employ our art for the relief of our patients? If I take the views presented on one side, I know nothing at all about the liver; I do not even know that there is any bile ever thrown into the alimentary canal. I know nothing at all of its office-work in the system. According to the teachings I received as a student,

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and that I and most other members of the profession have been accustomed to use as guides in the administration of cholagogues, I feel, when I approach the bedside, that I am not in the dark—that I have something by which I am able to determine whether or not the liver is in a healthy or abnormal state —whether it is acting in excess, or not acting at all. This is a practical point of immense importance. Our object in assembling, year after year, in this capacity, is a practical one. If the position assumed on one side is true, we must ignore the use of mercury. It has not been pointed out to us what effect it will have if we do administer it. The discussion seems to have turned entirely upon this question—whether the liver secretes bile or not? If there is no bile secreted, as is asserted on the one hand, then it is plain that there is no agent calculated to produce it. I have long since noticed the fact that there is a revolution going on in the medical profession on this subject. There can be no doubt whatever that this is the effect of the extensive and indiscriminate use of mercury by incompetent hands. However important a reform may be, there is a point where it must stop. Suppose a practitioner is called to a patient who is suffering from iritis. Unless some means is employed very soon to stop the progress of the malady, the result will be the destruction of the organ. He administers a small quantity of mercury, and sees the disease disappear. While we are cutting off abuses in practice, do not let us go to the other extreme, and say that mercury is useless. Mercury has been greatly abused. It is, in fact, when skillfully applied, one of the very best remedial agents we have; while, at the same time, in the hands of ignorant or careless men, it is one of the worst. Mercury is put down as one of the most powerful antiphlogistic agents we have, and I have not yet found anything to overthrow that position. It is not used, I am aware, to the extent that it has been. That is proper. We understand its effects, and the conditions under which it should be administered, much better now than we formerly did.

I have made these remarks for the purpose of calling your minds more toward practical results. In all cases, particularly where we are unsuccessful, we want the consolation of knowing that we treated the cases with the very best means known to the profession.

John Moffett moved that the report be referred to the Committee on Publication. Carried.

T. B. Harvey moved that a committee of five be appointed to examine and report upon voluntary papers. Carried.

The Chair appointed the committee as follows: T. B. Harvey, John Rea, A. G. Preston, P. W. Payne, and J. H. Woodburn.

J. F. Hibberd moved, that inasmuch as, upon a statement

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that Dr. H. C. Cole, of Kokomo, had notice of charges against him, and had not appeared, the Society had acted upon said charges in his absence; and he had since appeared, that the action of the Society be reconsidered. Carried.

The Secretary then read the report of the Committee on Ethics relating to Dr. Cole's case.

H. C. Cole made a statement in which he claimed the paper to be an original one.

H. Charles moved that the whole matter be recommitted to the Committee on Ethics, with instructions to hear Dr. Cole in his own behalf. Carried.

T. B. Harvey, from the Committee on Voluntary Papers, submitted the following report:

The Committee appointed to consider voluntary papers, respectfully report that they have examined a paper on Foreign Bodies in the Air Passages, by J. R. Weist, M. D.; one on Tracheotomy in Cynanche Trachealis and Diphtheria, by R. E. Haughton, M. D.; one on A Case of Dropsy, with Remarks, by John Moffett, M. D.; and one on Female Doctors, by Dougan Clark, M. D.; and report the same, in their opinion, worthy of being heard in full by this Society.

THOS. B. HARVEY, *Chairman.*

The report was concurred in.

J. R. Weist read a paper entitled A Contribution to the Statistics on the Subject of the Presence of Foreign Bodies in the Air Passages.

J. H. Woodburn moved to refer Dr. Weist's paper to the Committee on Publication. Carried.

R. E. Haughton moved that Dr. Weist be allowed to make use of the materials of his paper, after its publication with the Transactions of the Society. Carried.

W. R. Winton, from the Committee on Ethics, reported that Dr. Cole had pointed out to the Committee, in the address of Dr. Bissell, several sentences which were not found in Dr. Cole's paper; but none in the paper, of any importance, which were not found in the address. The Committee saw no reason to change the recommendation in the report.

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The question being on re-affirming the action of the Society, approving the report and expelling Dr. Cole, it was carried unanimously by a rising vote.

W. R. Winton, from the Committee on Credentials, made a further report, as follows:

The Committee on Credentials have considered the certificate of G. V. Woolen, M. D., as delegate to this Society from the City Hospital of Indianapolis, and while they do not feel authorized by article third of the Constitution to admit any but representatives from county or local medical societies, recommend that such a change be made in the Constitution as to admit, hereafter, delegates from hospitals located in the State.

W. R. WINTON, *Chairman.*

Wm. Lomax moved to adjourn till two o'clock this afternoon. Carried.

AFTERNOON SESSION.

2 O'CLOCK P. M.

The Society met pursuant to adjournment. The President, Dr. Kersey, in the chair.

James F. Hibberd offered the following resolution:

Resolved, That every practitioner of medicine, of whatever kind or character, in the State of Indiana, should be placed under a legal obligation to pay five dollars annually, as a contribution toward sustaining, pecuniarily, the organizations that are endeavoring to advance medical science and art.

On motion, it was, for the present, laid upon the table.

The special order for the hour being the election of officers, Wm. Lomax, from the Committee on Nominations, reported the following as officers for the ensuing year:

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PRESIDENT,
J. S. BOBBS, M. D., Indianapolis.

VICE PRESIDENT,
R. B. JESSUP, M. D., Vincennes.

SECRETARY,
L. D. WATERMAN, M. D., Indianapolis.

ASSISTANT SECRETARY,
G. V. WOOLEN, M. D., Indianapolis.

TREASURER,
W. B. LYONS, M. D., Huntington.

LIBRARIAN,
WM. WANDS, M. D., Indianapolis.

They were unanimously elected.

George Sutton, of Aurora, read a report on Cholera.

J. H. Woodburn moved that Dr. Sutton's report be referred to the Committee on Publication. Carried.

W. S. Haymond, of Monticello, read a report on The Collapsed Stage of Cholera.

R. A. Curran moved that the report of Dr. Haymond be referred to the Committee on Publication. Carried.

G. Sutton said:

I would like to ask Dr. Haymond, as he seems to think cholera is a blood disease, if it can be produced so easily and so suddenly by a merely exciting cause, and yield so readily to remedies taken into the stomach? How could merely irritating substances taken into the stomach produce a blood disease? A blood disease is one in which the blood is undergoing a change in itself, and is not such a disease as will yield at once to an agent taken into the stomach.

J. F. Hibberd made an inquiry of Dr. Haymond as to his opinion of the cause and origin of cholera.

W. H. Haymond stated that his position was that the germ of the disease was an entity; and that the disease arose from its propagation.

J. F. Hibberd discussed the idea advanced by Dr. Haymond as follows:

In all these investigations we must not forget that all these diseases, such as cholera, scarlatina, &c., have each a distinct and specific cause. The

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appropriate cause of one disease, whatever it may be, never gives rise to another. I have a dim faith that, in the future, we shall come to know that many of those diseases that have a period of incubation, if not actually organized entities, have at least so many of the characteristics of organized entities, as to be in some sort living beings. But at the present stage of our knowledge, it seems to me that it is going too far to declare that living beings are the cause of cholera or any other disease of that class; but especially is it going beyond what we know, and beyond the limits of prudence, to base upon that idea our system of pathology, and regulate thereby our system of therapeutics. Undoubtedly the time will come when we will have a great deal more light upon this subject than we have now.

In regard to the best mode of treatment in the collapsed stage of cholera, it is my conviction that, so far as internal remedies are concerned, the best thing we can do is to let it alone. Owing to the condition of patients in that stage, whatever remedy may be administered internally, it will of necessity lie powerless in the stomach, until a change takes place in the condition of the patient. Then the remedy will act; but what will, in that new state of the system, be its effect? It may, and in very many cases does, do mischief. The situation of the patient, in collapse, we might call "animation inanimate," not dead, yet absolutely so as far as concerns any remedies we can give him through the stomach. If there is any leading indication that we can safely follow, it is to be found in the wishes of the patient himself; let him have, if anything, some of those articles which, while in that condition, he so ardently craves. Further than this, so far as the stomach is concerned, I do not believe we can do anything. If we give anything more than that, it will lie inactive in the stomach, until reaction comes on, and then may do mischief. When we have not a reasonable certainty that we can do good by means of remedies, leave the matter in the hands of Nature.

John Moffett, of Rushville, read a report on Spotted Fever.

J. S. Bobbs moved that the paper on Spotted Fever be referred to the Committee on Publication. Carried.

Jas. F. Hibberd moved that W. Lomax be continued as the Committee on Surgery until the next annual meeting, he not having been able to complete his report for the present occasion. Carried.

J. S. Bobbs moved that Dr. George Sutton be requested to continue his observations and investigations of Cholera, and report at the next annual meeting. Carried.

J. S. Bobbs informed the Society that he had not been able

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to prepare his report on Inflammation in time for the present meeting.

Jas. F. Hibberd moved that, inasmuch as Dr. J. S. Bobbs had been chosen by the Society as its President for the ensuing year, he be relieved from the duty of preparing a report on Inflammation. Carried.

Dougan Clark read a volunteer paper on Female Doctors.

R. E. Haughton moved to refer the paper on Female Doctors to the Committee on Publication, with instructions to print it.

J. H. Woodburn moved to amend by adding to Dr. Haughton's motion the words, "with the understanding that this Society does not approve of the sentiments therein contained."

T. B. Harvey moved to lay Dr. Woodburn's amendment on the table. Carried.

The question recurring upon Dr. Haughton's motion to refer the paper to the Committee on Publication, with instructions to print, Dr. Woodburn said he hoped it would not pass; the profession was certainly not ready to approve such sentiments as that paper contained.

N. L. Wickersham spoke in favor of the motion. He said that the Society did not, by the mere act of ordering a paper published, approve or endorse anything contained in it. The motion was not to approve the paper of Dr. Clark, but to print it.

The motion to refer to the Committee prevailed.

R. E. Haughton offered the following resolution, with the recommendation that it lie on the table until the next annual meeting:

WHEREAS, Women are subjected to disabilities in procuring medical education, by reason of exclusion from the medical colleges of our country; therefore,

Resolved, That such disabilities, imposed by the action of such colleges, should be removed, and the admission of women, for the purposes of education, be allowed.

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Resolved, That the Medical Society of the State of Indiana is willing to recognize women as practitioners of medicine, subject only to the same requirements of education and observance of ethics as are required of men.

W. Lomax moved that the resolution lie upon the table, as suggested by Dr. Haughton. Carried.

The President announced the following Standing Committees for the ensuing year:

COMMITTEE ON ARRANGEMENTS.—John A. Comingore, James S. Athon, Thomas B. Harvey, John M. Kitchen, and H. F. Barnes.

COMMITTEE ON CREDENTIALS.—John Moffett, R. E. Haughton, Wm. Lomax, A. G. Preston, and D. D. Hall.

COMMITTEE ON FINANCE.—D. H. Oliver, H. G. Todd, H. P. Ayres, J. W. Pugh, and Dougan Clark.

COMMITTEE ON ETHICS.—W. C. Thompson, Joel Pennington, R. A. Curran, R. B. Jessup, and R. N. Todd.

COMMITTEE ON PUBLICATION.—L. D. Waterman, W. B. Lyon, W. F. Harvey, T. M. Stevens, and G. V. Woolen.

Also, the following Special Committees:

CHOLERA.—George Sutton.

SURGERY.—Wm. Lomax.

DISEASES OF FEMALES.—Theophilus Parvin.

OBSTETRICS.—G. W. Mears.

THERAPEUTICS.—F. S. C. Grayston.

ERYSIPELAS.—John A. Comingore and A. G. Preston.

PATHOLOGY OF DIPHTHERIA.—James F. Hibberd.

MEDICATED INHALATIONS IN PULMONARY DISORDERS.—W. S. Haymond.

The Secretary announced that all the papers present had been disposed of, with the exception of that of John Moffett, entitled "A Case of Dropsy, with Remarks," and that of R. E. Haughton, entitled "Tracheotomy in Cynanche Trachealis and Diphtheria."

J. A. Comingore moved to refer those papers, without reading, to the Committee on Publication. Carried.

J. H. Woodburn moved that the President be authorized to appoint the delegates to the next meeting of the American Medical Association, after the adjournment of the Society;

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and send their names to the Secretary for insertion in the printed Transactions of this session.

After being amended, by substituting Vice President for President, it was carried.

The following is the list of

DELEGATES TO THE AMERICAN MEDICAL ASSOCIATION FOR 1868.

NAME AND RESIDENCE.	NAME AND RESIDENCE.
V. Kersey, Richmond.	W. A. Pugh, Rushville.
J. R. Weist, Richmond.	A. G. Boynton, Elizabethtown.
B. S. Woodworth, Fort Wayne.	E. H. Crippen, Moscow.
Wm. H. Thacker, Fort Wayne.	James W. Martin, Lebanon.
Thos. B. Harvey, Indianapolis.	Henry V. Passage, Peru.
L. D. Waterman, Indianapolis.	Henry G. Todd, Danville.
A G. Preston, Greencastle.	L. R. Johnson, Cambridge City.
N. Field, Jeffersonville.	A. W. Reagan, Mooresville.
S. D. Day, Shelbyville.	Joseph Jones, Williamsport.
George Sutton, Aurora.	D. S. Layman, Huntington.
R. M. O'Ferrall, Lafayette.	L. Humphreys, South Bend.
C. Lomax, Marion.	N. D. Clouser, Hartford City.
S. W. Thompson, Evansville.	F. M. Hiett, Saybrook.
S. H. Charlton, Seymour.	J. M. Darnall, Kokomo.
P. W. Payne, Franklin.	Jas. Cochran, Spiceland.
John Rea, Newcastle.	G. W. H. Kemper, Muncie.

N. L. Wickersham moved that the thanks of the Society are due, and are hereby tendered to Dr. Vierling Kersey, for the able manner in which he has discharged the duties of President of the Society. Carried.

J. S. Bobbs, from the Committee on Prize Essays, announced as the subject of the Prize Essay for the ensuing year, The Causes, Nature, and Treatment of Cerebro-Spinal Meningitis; and recommended that the prize be one hundred dollars. The report was adopted.

Upon motion of William Lomax, the Society adjourned, to meet on the third Tuesday of May, 1868, in the city of Indianapolis.

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REPORT OF THE COMMITTEE ON PRIZE ESSAY

The Indiana State Medical Society offers a prize of one hundred dollars to the physician, resident within the State, who shall present the best and an acceptable, Essay on "THE CAUSES, NATURE AND TREATMENT OF CEREBRO-SPINAL MENINGITIS."

Competitors must send their Essays to J. S. Bobbs, M. D., Indianapolis, Ind., free of expense, before April 1, 1868. Each Essay must be accompanied by a sealed letter, containing the author's name and address, and inscribed with some motto or device, which must also be inscribed on the Essay.

The successful Essay will be the property of the Society; the unsuccessful ones, with the sealed letters unopened, may be obtained of the Secretary during the ensuing year.

J. S. BOBBS, M. D., Indianapolis, Ind.,
S. S. BOYD, M. D., Dublin, Ind.,
A. G. PRESTON, M. D., Greencastle, Ind.,
} *Com. on Prize Essay*

May 22, 1867.

Medical journals having circulation in Indiana are requested to notice the foregoing proposition.

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LIST OF OFFICERS AND MEMBERS

Of Auxiliary County and Local Medical Societies, 1867.

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A. B. Bradbury, Milton.	James F. Hibberd, Richmond.
T. F. Sweeney, Milton.	S. B. Harriman, Richmond.
J. Q. A. Robbins, Abington.	Elias Fisher, Richmond.
Nathan Johnson, Cambridge City.	Vierling Kersey, Richmond.
L. R. Johnson, Cambridge City.	Israel Tennis, Richmond.
S. D. Personett, Greensfork.	M. W. Hobbs, Richmond.
T. J. Courtney, Hillsborough.	J. H. McIntyre, Richmond.
S. S. Boyd, Dublin.	J. R. Weist, Richmond.
Harlan Harrison, Newport.	Edwin Hadley, Richmond.
A. B. Ferris, New Paris.	Dougan Clark, Richmond.
S. H. Kersey, Westville.	

GRANT COUNTY MEDICAL SOCIETY.

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C. Lomax, Marion.	George Egbert, Xenia.
J. Shively, Marion.	H. H. Beeson, Xenia.
L. Williams, Marion.	John W. Pugh, New Cumberland.
L. Hess, Marion.	H. D. Reasoner, New Cumberland.
A. C. Irwin, Jalapa.	A. Henley, Fairmount.
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B. Bartholomew, Danville.	Robert Lewis, Plainfield.
Wm. Hoadley, Danville.	L. H. Kennedy, Belleville.
J. T. Hall, New Winchester.	Risdon C. Moore, Belleville.
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J. T. Scott, Greentown.	J. Shirly, New London.
J. F. Henderson, Kokomo.	R. H. Buck, Kokomo.
—— Landers.	O. Richmond, Kokomo.
A. F. Dayhuff, Kokomo.	R. Q. Wilson, Kokomo.
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D. McClure, Jeffersonville.	R. W. Shaw, Utica.
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L. W. Beckwith, Jeffersonville.	D. H. Combs, Charleston.
G. W. Lewman, Jeffersonville.	S. C. Taggart, Charleston.
James A. Post, Jeffersonville.	A. J. Hay, Charleston.
F. A. Seymour, Jeffersonville.	H. H. Ferguson, Hubbard.
Thos. C. Mercer, Jeffersonville.	Robert Taggart, Slate Cut.

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(Rush, Henry, and Hancock Counties.)

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J. Riddle, Knightstown.	James Cochran, Spiceland.
John Wall, Knightstown.	—— Cain, Spiceland.
J. Stuart, Knightstown.	John Lewis, Ogden.
—— Whitesell, Knightstown.	—— Butler, Coffin's Station.
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L. J. Ham, South Bend.	S. B. Collitt, Walkertown.
L. Humphreys, South Bend.	—— Poffenberger, North Liberty.
J. C. Fletcher, South Bend.	S. Lanning, North Liberty.
J. C. Sack, South Bend.	D. Hoffman, North Liberty.
S. Eisenbis, South Bend.	J. Harris, New Carlisle.
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R. M. O'Ferrall, Lafayette.	George F. Beasley.
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PRESIDENT'S ADDRESS.

PHYSIC AND PHYSICIANS.

BY VIERLING KERSEY, M. D., RICHMOND, IND.

In conformity with established usage it becomes my duty to address you on some topic of my own choosing. Inasmuch as the greater part of my life has been devoted to the study and practice of our profession, I shall endeavor to acquit myself of the obligation by presenting a few thoughts touching the Science of Medicine and those engaged in its practice.

Human suffering naturally excites human sympathy, and prompts measures for relief. Hence the origin of medical knowledge, the source of its development in the past ages, and the assurance of its unlimited progress in the time to come.

Our profession, thus called into being by disinterested human sympathy, is, in its true nature and office, essentially vicarious; constrained by the highest sentiment of our nature to suffer with those who suffer, and labor for their relief.

Early attempts to relieve pain were of necessity strictly tentative and empirical—the nature both of diseases and remedies being wholly unknown. Even now intelligent and guarded empiricism is the chief means of extending and verifying our knowledge of remedial agents.

With the Science of Medicine, when merely a traditional myth, whose entire stock of positive knowledge might have been transferred to a dozen pages, it is pleasant and interesting to compare its present stupendous proportions.

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In plain speech—the Science of Medicine is not a science. It is much too broad and complex to admit of this designation. It is an art—the highest and most difficult of arts—the art of applying with judgment and skill, in the prevention, mitigation, and cure of disease, a vast fund of knowledge derived from a thorough mastery of an extensive circle of sciences.

Anatomy, physiology, chemistry and botany—approaching, in exactitude, mathematical science—require to be thoroughly known by the physician; while geology, mineralogy, meteorology, the physical and metaphysical sciences, all make invaluable contributions to the stock of his knowledge. All these sciences—and we say nothing of the literary and mathematical training, and the knowledge of two or three foreign languages, so nearly indispensable to their ready acquisition, and otherwise so useful to the physician—are to be regarded as so many steps toward a fitness to engage in the study of the healing art, wherein all departures from health are to be learned through their causes, symptoms, lesions, and terminations; and its restoration and preservation through the discreet application of remedial agents, and hygiene.

Thus, from a condition in which nothing was known of the laws of nature, of health, of disease or remedies, we have reached a position wherein it is not to be supposed that any one man can achieve a thorough personal mastery of all the sciences tributary to Medicine—much less that he can possess a competent knowledge of all these, and, at the same time, the highest degree of skill in every department of the healing art. This may be safely affirmed of men of the best natural capacity, under circumstances most favorable to the acquisition of knowledge, during a life of uninterrupted application. What, then, is to be expected where the physician is obliged to practice every department of his profession, do a large share of his own pharmacy, and take long rides almost every day of his professional life to get to his patients? Yet this is the condition of most of the physicians of our State. An active practitioner, in such a field of duty, finds it impossible to keep his rank in the progress of medical science. He acquires knowledge, it is true, by observation and experience; but he is likely to forget what is not called into frequent requisition, and is really not in a condition to make the most of his own personal experience—for this can be done only where we are able to verify both diseases and the effects of remedial agents on diseases thus clearly defined.

It is true that many physicians for years in laborious practice, with rarely an hour of unbroken study, prompted by a high sense of duty, use every available opportunity to improve their knowledge of disease by examination of the dead body. Yet it may well be confessed that the results usually reached in this direction are greatly short of what they should be. Accurate and complete, and therefore highly instructive examinations of this kind, are probably rarely made among us—indeed they require a kind and a degree of

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skill not to be acquired, and scarcely to be retained, in the midst of a laborious country practice.

In cases of death from suspected violence or poisoning, the courts—in order to secure the ends of justice—require, and should be able to command, the highest degree of certainty that science can afford. The decision of such cases depending almost exclusively on medical testimony, the profession should never feel at ease, nor as adequately filling its place in society, till we are able to designate at least one physician in each county of the State, thoroughly familiar with analytical chemistry, toxicology, histology, morbid anatomy, microscopy, and medical jurisprudence, and every way fitted for the discharge of this class of responsible obligations in the most enlightened, scientific and trustworthy manner. Neither should we rest till satisfactory provision is made for the certain and ample compensation of all such service. Were each county supplied with even one accomplished expert in these departments of medical knowledge, the value of examinations of the body, in cases of death from obscure and remarkable diseases, might be greatly enhanced; our knowledge of morbid anatomy, and diagnosis in certain difficult cases essentially advanced; and many wants ably supplied which are now often keenly felt.

The propriety of division of labor in the practice of medicine, has long been obviously indicated; and the necessity is constantly and rapidly augmenting. The thorough practical mastery of what is already known in the several departments of medical science, and the extension of the bounds of useful knowledge in each, alike demand this measure. Any community able to pay for the highest attainable skill in every branch of practice, will be likely to appreciate the value of division, and favor its speedy introduction.

Our rank in the profession can be maintained only by unslumbering industry. We cannot overlook the fact that, in the item of progress, medicine is in full sympathy with the fast age in which we live. No preceding era, in the entire career of medical advancement, has accomplished anything like so much as our own. This seems to be due, in a great measure, to a clearer comprehension of some of the laws of nature, whereby inquiry has been led, with unexampled directness, to the richest of mines, as yet by no means fully explored; and thus the immediate future appears, to all human sagacity, teeming with promise of the most rapid and substantial progress.

We are yet, probably, wholly unacquainted with many of the laws of nature. It is not probable that all these laws have been called into operation at any time during the lapse of the past ages. The cultivation of the wilderness, the complex operations of commerce, the immense agency of the artisan, the progress of civilization, and the increase of population, when carried to the extreme, may evoke laws hitherto entirely inoperative — processes hitherto unknown—and thus originate or eradicate infectious and pestilential disorders in the animal and vegetable kingdoms. The appearance of new

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epidemic disorders at various epochs, some of them recent, renders it more than probable that certain laws of disease have been brought into active play for the first time within the memory of men now living. It is not to be supposed that all possible combinations among elementary substances have already been effected. We cannot foresee what scourge, in the way of new disorders, nor what boon, in the way of new remedial agents, the future may thus evolve from the storehouse of nature.

A century has scarcely elapsed since intelligent chemists believed themselves able to annihilate many kinds of matter. Improved instruments, more thorough methods of investigation, and closer scrutiny, corrected this error, and established the fact that matter is indestructible, notwithstanding it is capable of assuming an endless variety of forms. The recognition of this fact led to the rapid development of chemistry, on a basis of certainty second only to the science of numbers.

It remained for the men of our own age to educe the still higher law of science, that *force is an imperishable unit*—derived from a single source—and although capable of an infinite variety of modes of being, in the operations of nature and of art, it still remains the same persistent, indestructible, imperishable unit, whether active or quiescent. Light, heat, electricity, magnetism, chemical affinity, catalysis, gravitation, mechanical motion of every kind, germination, development and decay, animal and vegetable, are among the endless modes of force, and, to a great extent, convertible into each other. It is scarcely possible to over-estimate the value of this generalization, with respect to its influence on the progress of science, and especially on medical research.

In conformity with these indications of science, and the results of spectrum analysis, doubts are raised as to the essentially elementary nature of the so-called chemical elements. Prof. Heinrichs thinks it probable that they may all be resolved into a single primary element, and "hopes to prove that the unity of matter is as real as the unity of force."

In view of the high culture requisite to an enlightened comprehension of medical science, it is obvious that a very small proportion of our young men are sufficiently educated to engage in its study with a prospect of creditable success. Talent, of a cast to give assurance of professional eminence, is still more rare than learning.

Encouragement to enter the medical profession, without thorough qualifications, natural and acquired, should never be given. The profession has already suffered, perhaps, more deeply from laxity in this quarter than from any other source. It seems to be the pride of some physicians to be surrounded with medical students. Where this is the case, every crude desire to study medicine is likely to find encouragement; aspiration is assumed to imply qualification; and we thus have students of medicine who never studied anything else. What more melancholy sight than a young man of ordinary

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natural capacity, unskilled in his mother tongue, and innocent of all science, plodding his dreary way through the text books of medicine; muttering technical phrases beyond his comprehension, and dreaming that their smattering acquisition is the limit and consummation of his studies! A common regard for the well-being of the student, the profession, and humanity at large, demands that every such, and somewhat such aspirant should fail to find a preceptor in our State.

It is to the physician in his private capacity at home that we are to look for a primary and much needed step toward the genuine elevation of the profession. If every physician of the State does all that can be done to maintain thorough fitness for the discharge of his high obligations, with scientific skill and becoming simplicity, and guards against the accession of unworthy members, by declining all students not fitted by nature and training to adorn a profession luminous only in the convergent light of literature, art and science, we shall make steady progress toward a higher standard of worth; and it would be unfortunate for ourselves, and the world, if anything short of this could afford a position of seeming eminence or repose.

There is, it is true, in charlatanry an element of temporary success, (a fact, to our shame, not wholly ignored by the regular profession.) It is sure, however, to work degradation and injury wherever it exists. A life of unbroken pecuniary prosperity, won by this means, is to be reckoned among the meanest of failures—deeply injurious to the profession at large, and essentially damning to the party concerned. Charlatanry is to be regarded as an element of character, rather than a scheme for success, in medicine. It is by no means confined to our profession. The regret is, that it is not wholly excluded. It invades every department of society. Not unfrequently it may be seen, winning temporary applause, even in the pulpit. Wherever it abounds, truth and honesty are simply out of the question, and should not be looked for.

Medical statistics owe their value to their accuracy and completeness. Without these qualities they are the most pernicious element that can be employed in medical reasoning, and should at once be rejected. But it may be safely assumed that they are often essentially deficient in these respects. Correct diagnosis sometimes baffles the greatest skill and care; but these qualities, in a high sense, are not always brought into requisition; and thus the value of statistics is undoubtedly often essentially impaired. Some reports carry with them a manifest tincture of prejudice—unquestionably a fruitful source of error in statistics. A mere dash of constitutional charlatanry is enough to degrade statistics below the level of ordinary guess-work. Unqualified diagnosis of grave disease, at a very early stage of illness, may lead to extensive error in statistics. For instance: malignant cholera, often arrested by appropriate means in the premonitory stage, when diagnosis is far from clear, is easily recognized and scarcely amenable to any kind of

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treatment when fully developed, and thus affords abundant opportunity for the vitiation of statistics. In those cheering numerical successes sometimes exhibited in the treatment of this malady, it is extremely probable that the cure often precedes the attack. The utter inefficiency of the vaunted remedy, in other hands, lends additional support to this probability.

But setting aside all sources of falsification in statistics themselves, (and they are numerous and fruitful,) other difficulties, of almost equal significance, remain. Different visitations of the same disease are often very different in fatal tendency. We sometimes see a severe and extensively prevalent epidemic of dysentery, scarlatina, or the like, amenable, in almost every case, to an appropriate method of treatment; yet, perhaps in the next visitation of this disease our favorite treatment proves inapplicable, while a different, perhaps an opposite, course succeeds. At another time death may occur under any management, with shocking frequency. Hence, it is obvious that correct and complete statistics of any one of these visitations, or of a number including every grade of fatality, will shed a very uncertain light on the prognosis and treatment of any pending seizure, and can scarcely be regarded as possessing any kind of value as indicating the proper management of a single case. The age, constitutional peculiarities, habits of life, native vital force, severity of attack, grade of fatality of the prevailing epidemic, and a full appreciation of all the symptoms in each case, are our chief guides to accuracy in prognosis and success in treatment. But many of these considerations are imperfectly presented, or wholly omitted, in medical statistics; hence, when they are greatly relied on as guides, there is an obvious tendency to recognize disease by its name, to contemplate it in the aggregate, and subject it to a routine of stereotyped practice, without that vigilant regard to the special wants of each individual case that constitutes so essential an element of success in the practice of our profession.

Touching the value of frequent professional intercourse, there is, perhaps, theoretically little difference of opinion among physicians. That properly conducted medical societies, from the smallest up to the largest, are essentially useful to all regular working members, is, perhaps, admitted on all hands; yet this fact seems to be practically ignored to a very great extent. An indefinite proportion, perhaps much less than half, of the physicians of the State attend medical meetings whenever it is reasonably practicable, and contribute what they can to their interest; while the majority seldom, or never, attend. It would be well for each one of us to determine, with distinctness, for himself, the ground of negligence in this matter. Is it indifference to our own personal interest, indifference to the interest of the profession at large, or does it arise from a conviction that the time may be otherwise better employed? In vindication of habitual absence, it may be said to be doubtful whether even our State Society is worth attending; that its meetings are generally small and dull, and never highly interesting; of its transactions throughout,

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that it would be difficult, in any other market, to find a like amount (and this is small in the extreme) of medical literature of so little intrinsic value; besides the cost is four-fold that of a standard article. If we admit all this, what follows? Only this: it affords a reliable index of the state of the profession in Indiana — not of a few physicians, but of the entire brotherhood, and for which we are all responsible. If we do not like the standard, it is for us to improve it. It is our duty, our privilege, our business. It will never be done for us, nor by a small fraction of our number. There is in this enterprise a special opening for each and every one of us to work. Our interest, our duty, our self-respect, nay, even our pride, demands earnest and persistent coöperation for the promotion of intelligence, usefulness, dignity and harmony in the profession. The opposite course of mutual repulsion and neglect, will not fail to work still further degradation. Failure to respond to motives so urgent, personal and inspiring, as bear on every member of the profession, should suggest a grave doubt whether professional progress really is an object of our earnest pursuit.

In any village of three or four physicians, a club meeting of two hours, once a week, for the discussion of matters of professional interest, conducted in a spirit of candor and liberality, and kept up as one of our life-long professional obligations, will yield a rich reward to all concerned. If due courtesy and forbearance be sometimes wanting, what so likely to secure their ultimate practical exercise as persistence in professional intercourse?

No city or county can well forego the advantages of four or more medical meetings every year. If suitable measures are adopted to render such meetings interesting, and they are conducted with a single view to the general good, they will scarcely fail to unite in cordial coöperation all worthy members of the profession. Local and county meetings could not be carried forward with intelligence and spirit, throughout the State, for ten years, without infusing a new interest, and establishing habits of methodical labor, of essential value to the profession. This Society would thus be enabled to secure correct annual reports of the diseases of every portion of the State, with such other facts, topographical, meteorological, and telluric, as would shed light on their nature and origin. A simple and faithful record of this kind would increase in value from year to year. Its collection and preservation is an object entirely worthy our constant care, especially in view of the fact that accurate and positive knowledge of the phenomena of disease, and of surrounding circumstances, is the first rational step toward both prevention and scientific cure.

We are without a medical journal in the State — a fact we probably have no reason to regret. State lines in America, national boundaries and broad oceans on earth, are getting to signify little in the way of restraints on the diffusion of literature; and there is reason to believe that this little will become less and less for an indefinite period to come. When men go into mar-

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ket for intelligence, as for other commodities, they should forego sectional prejudices, and get precisely what they need. This rule applies with special force to medical intelligence. Bad medical literature should never be exposed to sale in any market. But if men will place it in market, that is no sufficient reason why it should be purchased.

We are already supplied with more periodicals than we can afford to read, though gratuitously placed on our tables. Were the profession what it should be, it is obvious only first class journals could subsist. In that case, perhaps half the present number would perish during the current year. Were all the editorial talent of the country concentrated upon a very few journals, they would be sure to secure, and richly repay, the perusal of every living member of the profession. The editorial compensation, in that case, would be such as to insure the performance of the service in the most thorough, independent and trustworthy manner. And thus an enlightened and judicious guardianship of medical literature would furnish the physician precisely what he needs, and, at the same time, guard him against the painful necessity of reading (not to speak of the minor evil of purchasing) many articles, nay, many whole volumes, that should never have been written.

But if we had a periodical of our own, could we reasonably expect it to reach mediocrity in point of value? Certainly not, without rare ability and great exertion on the part of its conductor. Even with these, it is difficult to see how it could be done. An abundant supply of well digested medical facts, not otherwise before the profession, would be indispensible to success, and this would have to be sought mainly among the physicians of our own State. But, in the aggregate, we are to be regarded as a few thousand laborious country practitioners, not accustomed to authorship, not in the habit of so much as carefully recording our own valuable experience; without those bonds of close professional association which place the observations of each within the reach of all, and without a general hospital, where medical facts accumulate rapidly, under the eye of a single observer. It is doubtful whether we have a dozen physicians of extensive practice who have regularly kept accurate notes of disease, its treatment and results, for a score of consecutive years, anywhere in the State. It is thus apparent that the great mass of medical knowledge among us lies quite beyond the reach of the journalist, beyond the reach of the profession; and, in the character of an authentic record of medical facts, it may be said to have already perished. Neither is this unfortunate state of things to be obviated by the establishment of a medical journal. It demands a reformation much nearer home.

A call for original communications in support of a State medical journal, would doubtless stimulate production and secure a liberal supply, and there is little reason to question that this supply would, in the aggregate, be sufficiently original; but that it would contribute largely to the stock of positive medical knowledge, there seems to be good reason to doubt. But this is pre-

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cisely the kind of medical literature we want. We have in the profession, already, writers enough and to spare—smooth and scholarly—engaged weaving, indiscriminately, the apochryphal with the reliable—the mythical with the real—evincing sufficient lack of judgment in selecting, and of industry in the verification of, their facts. Let us not, then, take measures likely to stimulate enterprise in this branch of industry, by getting up a local medical journal that we do not really need. Rather let us preserve correct notes of such valuable facts as fall within our reach; place them in common stock for preservation and classification, resting assured that they will ultimately find publicity equal to their worth in existing medical journals of established value.

We are also without a medical school in the State—a fact to be seen only in the light of rare good fortune. Not that medical schools are to be regarded with aversion. Far otherwise. If there be institutions in our country, around which the pleasant memories of the profession cluster and love to linger, they are our medical schools. If there be names embalmed in the incense of youthful gratitude and veneration, maintaining fresh and unabated through life their magic power to warm and stir the spirit, they are the names of our distinguished teachers, Warren, Chapman, Mott, Dunglison, Bell, Mussey, Locke. Though nearly all retired to the inner temple, they are still in our midst, enshrined in memory—elate, flush, animate—the living impersonations of eloquence, genius, learning and worth. May the halo about them never be lifted!

But what do we want with a medical school in Indiana? Surely nothing, unless it be established on a scale and basis to compete in excellence, in eminence, in every appliance and means of instruction, with the very first schools of the age. It should assume at once a high rank among our positive luxuries, for it could not possibly be regarded in the light of a necessary institution. It should be richly endowed, placing it quite beyond the influence of pecuniary considerations, and thus commanding teachers of the first order—vigilant guardians of professional character; providing against unworthy membership, by enrolling only those of unquestionable fitness, and conferring degrees on merit alone. Such an institution will be welcome whenever it comes.

But a medical school destined to struggle for bare existence, constrained to admit every applicant, merely for the pittance he pays; capable of competition mainly in the cheapness of its teachings, and its facility in conferring degrees bearing little significance as testimonials of merit—such an institution would unquestionably prove an essential calamity to the profession and people of the State, transient enough, no doubt, as a medical school, but more enduring as a monument of professional indiscretion.

When physicians in their private capacity, and teachers of medical schools, shall come to an adequate realization of the fact that we are, each and all,

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in the position, ex-officio, of guardians, *in limine*, of the profession; that we are responsible before Heaven, and in the eyes of men, for its shortcomings in capability, sterling intelligence, and usefulness to mankind; and when we become careful to discharge the various obligations of this high trust with fidelity and integrity—then may we look for the dawning of our day. But while reckless indifference to these living interests and sacred obligations reigns rampant, regardless of rank in the profession, may Nemesis go on, as she inevitably will, serenely heedless of deprecation, distributing the full measure of her righteous penalties inexorably, relentlessly, inflexibly.

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BILE.

BY JAMES F. HIBBERD, M. D., RICHMOND, IND.

I propose at this time to defend and substantiate some points succinctly stated in a communication I made to this Society two years ago, and which were, in an essay by Dr. Kersey last year, assailed as not being in accordance with the teachings of science.

In this paper I shall present and discuss four propositions, viz:

1. The source of the bile.
2. The function and destiny of the bile.
3. The agency of the bile in the color of the fæces.
4. The cholagogue powers of the mercurials.

Is the bile derived from the blood carried to the liver by the hepatic artery, or from the blood carried to the liver by the portal vein?

Perhaps the more common opinion among professional men is that the bile is secreted from the portal blood. It is not important that we inquire whence this opinion came, or when it originated. What we want to determine is whether or not it is true.

Carpenter (Hum. Phys. p. 378,) says the hepatic artery has an area of only one-eighth of that of the portal vein, and some have inferred that it is inadequate to supply the blood from which the bile is secreted; but the groundlessness of this inference is apparent when we reflect that the amount of bile secreted by a medium-sized man is about three pints a day, and that this is less than one drop every three seconds, or twenty drops a minute—a quantity certainly within the capacity of a vessel much the inferior of the hepatic artery in size.

Without consuming time to hunt up and discuss what may have been advanced in support of the affirmative of the proposition that the bile is secreted from the portal blood, I will proceed at once to give facts and arguments that must, I conceive, convince that, in the present state of science, we cannot avoid the conclusion that the bile, like other secretions in the human system, is derived from arterial blood.

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The double origin of the liver is not controverted by any embryologist within my knowledge, and, therefore, I need not cite authorities to prove it. And we are fully warranted, by the facts presented, in adopting the views of those recent histologists, who assert that the one part, to-wit: that developed around the common trunk of the umbilical and portal veins, is a parenchymatous mass, without duct, and of the nature of a blood-gland, analagous to the spleen, etc.; and that the other part, to-wit: that sprouting up from the intestine in a system of branching anastomosing tubes, and ramifying through the former part, is a true secreting gland, analogous in its structure and function to the salivary and similar glands.

We must not permit the intimate intertwining of these two glands to cloud our vision to their distinct origin and dual nature. They are, in these particulars, as markedly separate as the spleen and the pancreas, their regional neighbors. Carpenter, in his Comparative Physiology, (page 424,) informs us that in the invertebrate animals these two parts of the liver are not only distinct in the embryo, but that they are not conjoined in the adult. Let us remember, also, that in the human embryo the blood-gland part of the liver is confined exclusively to the venous system, and that the secretory part is confined exclusively to the arterial system, in their vascular relations. Attention to these points will materially aid us in determining the relation of these glands to the circulating fluid, in their mature and closely united condition.

In the invertebrate animals the liver is supplied with blood from the principal systemic artery only. This supply is continued in the vertebrated classes through the hepatic artery, but in addition there is, also, the larger amount arriving through the portal vein. (Carpenter, Comp. Phys., p. 425.) In mammalia the portal blood is drawn from the digestive apparatus and the spleen; but, in birds, fishes and reptiles, it consists not only of this blood, but that, also, which is returned from the pelvis and posterior extremities, so that portal blood is not the same in all even of the higher order of animals.

In adult man, notwithstanding the close relationship of the secreting and assimilating portions of the liver, the supply of blood maintains the same distinction as when the two portions were separate in the embryo—the portal blood being distributed to the parenchymatous or assimilating portion, and the arterial blood to the tubular or secreting portion. This is abundantly verified by Carpenter, (Comp. Phys., p. 426, and Hu. Phys., p. 376,) Morel, (Hum. Hist., p. 132,) and Robin, (N. Y. Med. Jour., March, 1867, p. 432.)

If, therefore, we have, in some inferior animals, livers that secrete bile with only arterial blood to supply them; if we have, in man's liver, a double organ with a double function, one portion supplied with venous blood exclusively, and having the structure and performing the function of a true secreting gland by pouring out the bile; and if, furthermore, in certain diseases, as, for example, cirrhosis, where the part to which the portal vein is distributed becomes disorganized and incapable of performing its function,

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the secretion of bile still goes on, (Robin, op. cit.,) have we not testimony, more satisfactory than usually goes to the support of medical theories, that the bile is derived from the arterial, and not from the portal blood?

But I have more direct testimony than the foregoing to offer. We have numerous instances in man where no portal blood was supplied to the liver, and yet the bile was secreted. Müller (El. Phys., p. 368,) refers to two cases where, by a malformation, the portal vein entered the vena cava, not sending its blood through the liver, and nevertheless bile was secreted. Carpenter (Hu. Phys., p. 378,) also says that several cases are on record where the portal vein terminated in the vena cava, without ramifying through the liver, and yet the bile was secreted. Dunglison (Hu. Phys., 8th ed., vol. i, p. 539,) gives three cases of malformation and four of disease, whereby the blood of the portal vein was entirely cut off from the liver without arresting the secretion of bile. In one of the pathological cases reported by Dr. Gintrac, the author found such changes in the liver as led him to assume that the portal blood contributed to the nutrition of the liver, but was not essential to the secretion of bile. Later researches have explained to us what was then obscure to Prof. Gintrac, viz: that the portal blood, being normally distributed to the assimilating department of the liver, that part atrophied for want of physiological action when its supply of blood failed, while the tubular secreting department of the viscus, fully supplied with arterial blood by the hepatic artery, maintained its integrity and continued its functions.

Two hypotheses have been advanced by those who believe the bile to be derived from the portal blood, to account for the secretion of bile by a liver to which the portal vein carries no blood: 1st. Ritzius presumes that where the portal vein is impervious, some portal blood might reach the liver through the anastomoses of some abdominal veins; and, 2d. Kiernan, when convinced that the liver still furnished bile when no portal blood entered it, modified his views so as to declare that it was *venous*, and not necessarily *portal* blood that furnished the bile; and the blood of the hepatic artery, after nourishing the liver, was distributed to the secretory apparatus of the liver, and furnished the pabulum for the bile.

These explanations are too far-fetched and sophistical to exert any force in resisting the conclusion to be drawn from the facts presented; and these facts very clearly show that portal blood is not necessary to the formation of bile.

Resting the case, therefore, upon the testimony of to-day, we cannot avoid the conviction that the arterial blood affords the pabulum for the preparation of bile: and here the affair must abide until further investigation puts us in possession of additional facts that shall affirm or deny this conviction.

The next point for consideration is the function and fate of the bile. Long ago the belief was almost universal that it was directly excrementitious, and

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most of the pathology, and much of the therapeutics, of that day were based upon this idea. Nor is it difficult to find men of reputation now, who declare the bile to be directly excrementitious. Both Draper (Hu. Phys.) and Antisell (Am. Jour. Med. Sci., 1864,) make this declaration, the latter led to conviction by his own dialytic analyses of fresh human bile from a number of diseased subjects. Carpenter, (Comp. Phys., p. 430,) Liebig, Todd and Bowman, (Phys. Anat., p. 602,) and perhaps others, are of the opinion that the bile is indirectly excrementitious. They hold that the secretion of bile serves to depurate the blood; that it is then decomposed in the intestine and re-absorbed into the circulation; the sulphur to be eliminated by the kidneys, and the other ingredients through the lungs after oxidation. This is a very ingenious theory; but it puzzles one to see why nature, having the effete and offensive matter in the alimentary canal, did not send it on through, and eject it with other contents, instead of going to the trouble to refit it for absorbtion and sending it to distant emunctories for elimination.

It were an endless task to rehearse all the hypotheses that have been advanced in relation to the composition, the uses, and the final disposition of the bile. It appears to have been a favorite occupation with some eminent and excellent physiological chemists to put the bile through various and elaborate analytic processes to determine its proximate principles. Such manipulations always declared the bile a most complex fluid, of course; but of the substances resulting from these erudite investigations, no one could tell which of them were products of nature, and which were educts of the refined touch of the scientific analyst. Berzelius, long ago, suggested that bile was a much simpler substance than it was usually accounted, but his suggestion has not weighed much with many of his successors. Not only have some of these gentlemen made fine subdivisions of the bile by their chemical processes, but when the cunning of their hands failed them, they divided it into still other compounds by the assistance of their imaginations; and these they have hunted for where they thought such compounds ought to be, and having found something that answered the end, claim that they have discovered the destiny of the bile.

Without undertaking to exhibit the fallacy of conclusions thus drawn, or to follow the fancy of those who, abandoning the plain and simple, flounder among the intricacies of their own inventions, I shall proceed to give what I understand to have been established by various recent investigations in this department of physiological science.

In the examination of any secretion we must be careful to distinguish what is essential to, and characteristic of it, separating such from other constituents which, while they are constantly found in it, are yet, nevertheless, common to it and many other secretions. For instance: chloride of sodium is always present in bile, but it exists also in every other fluid and tissue in the body, and, therefore, claims no notice in such a review of the bile as we

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are now making. And the same for several other proximate principles of the bile.

It is in this sense that Lehmann, in his Chemical Physiology, (p. 169,) says there are two important principles of the bile, "one or several resinoid, and one coloring matter," and this, notwithstanding the extent to which he reports having dissected the bile into proximate principles in his Physiological Chemistry.

As we now understand that the cholesterine is an excrementitious substance, having its outlet through the liver, we must count it an essential principle of the bile, and add a third to Lehmann's two important principles. Therefore, for practical physiological and pathological purposes, the bile may be regarded as consisting of—1st. The bile salts, glyko-cholate and tauro-cholate of soda; 2d. the coloring matter, biliverdine; and, 3d. cholesterine.

Until recently the cholesterine, although understood to be the effete material resulting from the metamorphosis of nerve tissue, was not regarded as being important. But in 1862, Prof. Flint, Jr., (Am. Jour. Med. Sci., Oct.,) published his experiments, showing that it underwent chemical change in its passage through the intestine, and was found in the fæces in the condition of stercorine, to the extent of ten and a half grains daily for an average sized man. Flint also believes that the retention of the cholesterine in the blood has a toxic effect upon the system, producing a morbid condition, which he denominates cholesteræmia.

The bile salts, also, undergo chemical change, and loose their identity in the small intestines. What purpose they serve, and what becomes of them, is alike unknown. It is probable, however, that they are re-absorbed, and carried back into the circulation through the portal blood.

Biliverdine is in amount so small that its quantitative analysis has not yet been accomplished. Entering the duodenum with the other proximate principles of the bile, the biliverdine, like them, is rapidly changed, and is soon no longer recognizable in the progressing contents of the bowel. Because, however, of its broad-visual effect, it has, in the past, maintained a more important position than any other ingredient of the bile; indeed it was formerly held that the bile was present in its integrity wherever its coloring principle was found. Whether it is re-absorbed and carried back into the circulation, or undergoes a metamorphosis that, in destroying its identity as biliverdine, gives it some unrecognized character under which it is evacuated as an excrement, is a point that has, as yet, had no elucidation. While the investigations of Speir, (Prize Essay, Am. Med. Ass. Trans. 1864,) and perhaps others, make the latter view the more probable one, nevertheless there is nothing yet known that decides the question, and its full elucidation must, like many other physiological problems, await the more complete and successful investigations of a future day.

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This history of the bile is, in simple language, a confession of our pro found ignorance of its function and its destination: a truth that is much more satisfactory to the inquiring philosophical mind, than would be the invention of baseless hypotheses, or the promulgation of ingenious possibilities. Even if it were humiliating to confess our ignorance, it should be far less so than to attempt to deceive ourselves, or to mislead others, by an assumption of knowledge where we have no established facts for a foundation.

Another accepted proposition, that had no better foundation than loose inference, was that the color of the alvine dejections was due to the presence of biliverdine, and upon this vague generalization rested the popular professional opinion that the bile was constantly to be found in healthy fæces. That this opinion was erroneous, is shown by the results of all the investigations made in this behalf, undertaken for the purpose of eliciting the truth, with which I am acquainted. And even of opinions enunciated incidentally in the presentation of other points, one can find a great number acknowledging that the color of the stools is regulated by matters unconnected with the hepatic secretion, and that little or no bile is present in these excretions.

Todd and Bowman (Phys. Anat., p. 602,) inform us that the boa has a large liver relatively, but no bile can be detected in the fæces; and that, in carnivorous animals generally, little or no bile can be recognized in the excrements.

Berzelius (Phys. Anat., p. 602,) found nine parts of bile in one thousand of human fæces. Counting the daily fæces at five ounces, and the bile at three pounds, this would make only one part of the bile excreted with the fæces, in every one thousand parts furnished by the liver; a per centage so small that we had as well consider it nothing.

Dunglison (Hu. Phys., 8th ed., vol. ii, p. 849,) referring to the labors of Pettenkofer, remarks: "It would appear from the analyses of different observers that the fæces of both children and adults contain scarcely any evidences of bile;" and Kirke (Man. of Phys., Am. Ed., p. 216,) alludes to the analyses of Berzelius, Gorup-Besanez, Pettenkofer, and Bidder and Schmidt, as establishing the fact that the fæces of neither children nor adults contain any of the principles of the bile, except a very little of its "coloring, and some of its fatty matters."

Carpenter (Hu. Phys., p. 130,) refers to Rawitz's statement that chlorophyll, the green coloring principle of vegetables, passes into the fæces unchanged; then giving Marcet's analysis of the fæces, wherein the color is ascribed to "a coloring matter similar to that of blood and urine," and "an olive-colored substance of a fatty nature, excretolic acid," he proceeds with the following language: "Of the degree in which the bile, as a whole, nominally enters into the composition of the fæces, it is difficult to speak with precision. Its principal constituents can easily be recognized in the upper part of the small intestines; but the further we descend in the intestinal canal the less of them do we meet with; and in the contents of the large intestine, and in the

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evacuated fæces, they are seldom to be discovered." Carpenter (Hu. Phys., p. 362,) also declares that experiments of Bidder and Schmidt, with biliary fistulæ, show that "neither the solids of the fæces, however, nor those of the urine, were sensibly affected by the abnormal removal of the constituents of the bile."

Notwithstanding these recitals, and this ingenuous declaration, Carpenter enunciates the opinion that the color of the fæces is dependent upon the bile—an instance of the adhesiveness of a preformed conviction, despite of abundant testimony to the contrary presented by himself.

Lehmann (Chem. Phys., p. 178, et seq.,) finds in the bright yellow semi-fluid excrements of sucking children "bile pigment but little altered," as "also bile acids which give the usual reaction with sugar and sulphuric acid;" and declares that the *yellow color* of the excrements of adults is due to the presence of completely altered bile-pigment. Nevertheless he says that the fœtus, after five months, has, in the *small intestine*, a light-yellow mass, in which may be "found traces of resinous acids and bile-pigment," but in the large intestines, and in the meconium, "neither bile-acids nor bile-pigment is to be detected;" that ordinary fæces "contain only traces of altered constituents of the bile;" that after the use of iron the stools are black or green, and "the excrements also appear green after the use of indigo, black after partaking of whortleberries, light-yellow after the use of rhubarb, gamboge or saffron;" also, "that the stools are found to be black, chocolate-colored, or tar-like when blood is contained in them, and this arises from the upper part of the intestinal canal; so also the semi-liquid green excrements which are observed occasionally in typhus and other diseases, depend upon blood which is easily recognized by the microscope."

Upon the incidental testimony thus gathered from those who are resting under the idea that the fæces derive their color from the bile, we might safely rely to show not only that the bile is not the colorific agent, but also that the hue of the fæces emanates from many, and uncertain, sources. But I propose now to introduce direct testimony to substantiate the proposition that the bile is not the ordinary coloring agent of the fæces.

Prof. Armor, (N. Amer. Med. Chir. Review, Sept. 1859, p 864, et seq,) giving the views of another about the diarrhea of children, says: "From long and close observation, and careful analyses, Dr. Golding Bird arrived at the conclusion that green discharges have at least no direct connection with morbid biliary secretion, but that they are occasioned *by a morbid exudation from the internal mucous membrane.*" Prof. Armor, after vouching for the competency of Bird, and the accuracy of his analyses, adds: "I have observed the same discharges in the more malignant forms of scarlatina; and on submitting them to the test of the microscope, have always found broken up and distorted blood-corpuscles, together with an admixture of the elements of the blood, such as is seen in the green discharges of children."

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It must not be imagined that these gentlemen abandoned the idea that the liver was at fault in the diarrhea of children; so far from this, they undertake to show that it is the arrest of the secretion of the bile, and the congestion of the portal system, that brings about the condition of the intestinal mucous membrane that gives birth to the green discharges; and they quote Rokitansky in support of their position.

"Dr. Ward, in a letter published in the British Medical Journal for 1850, gives an account of a diarrhea occurring in his own person and lasting six weeks, during the whole of which period the stools were entirely free from color, and yet no evidence could be found of any biliary derangement, although the urine was frequently tested in reference to this point." (Trans. N. Y. St. Med. Soc'y, 1864, p. 276.)

Other cases of a like character are mentioned by Inman. (Ibid.)

Inman, (Ibid, p. 277,) also says: "I propounded the following question separately, without giving any reason for it, to four young medical men, all close observers, and diligent in their attendance in the dead house: '*Where does the brown color of the fæces begin?*' and the answer was the same from all, viz: "*In the colon.*" My own memory upon this point may possibly be treacherous. I therefore make the remarks interrogatively rather than affirmatively, and ask: 'Did ever any one see the contents of the small intestines of a brown, deep-yellow, or even a bilious hue? Did ever any one see, in cases of hernia, a brown fæcal matter flow from the bowel, if the small intestine only was implicated? Is it not a fact that the intestinal secretion is always whitish prior to its reaching the ileo-cæcal valve, and that it attains its fæcal character and color in the colon?' * * * * I examined with care the intestines of a man who had died in the Royal Infirmary rather suddenly. The canal was slit up from end to end, and it was clear that fæcification commenced in the colon."

Inman further quotes from a private letter written by Dr. Russell, who speaks of finding "fresh bile," in appearance, in fæcal matter in the colon, which evidently had been secreted by the intestinal follicles.

Perhaps every gentleman here has had more or less opportunity to inspect the contents of the small intestine, and I appeal to his own experience to affirm or deny the observations just presented in regard to the color of the intestinal contents below the duodenum.

I will close the testimony in this part of my subject by a reference to the declaration of Inman, (op. cit.,) "that he has known many cases of jaundice with complete suppression of bile, in which the fæces have retained their natural color;" and to the very striking and conclusive case of Bennett, (Pract. of Med., 3d Ed., p. 506,) where a patient having jaundice, purgatives were administered and followed by stools which the clinical record says were "well colored by bile;" calomel was also given, and it was followed by stools that were "green, of a dark color;" and yet the inspection,

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post mortem, showed that no bile could have passed into the bowel through the choledoch duct, it being impervious by virtue of an impacted gall-stone. Here, then, is a case where there was jaundice, the urine loaded with the coloring matter of bile, and after the use of purgatives, including calomel, the stools presented such characteristics to parties who were in the daily habit of examining such discharges, as to lead them to say, unhesitatingly, that they "were well colored with bile," and yet it is conclusively shown that no bile was present. Who, after this, shall say, on visual testimony alone, that any given matter does, or does not, contain bile?

By this rehearsal of testimony it is manifest that the fæces are colored by pigment derived from many sources—from the intestinal follicles, from blood, from divers drugs, and various articles of food. This certainly demonstrates that bile is, at least, not *necessary* to give the fæces color. And moreover it establishes the fact that the stools have been clay-colored for weeks, when the bile was, as far as could be determined, regularly and fully secreted; and, finally, it gives us the very conclusive evidence that the alvine evacuations presented all the so-called bilious appearances, to practiced eyes, when it was certain that no bile passed into the intestinal canal.

It must, therefore, be regarded as settled by our present knowledge, that bile is not the ordinary coloring agent of either normal or abnormal fæces.

Let us next inquire whether mercurials have any proper cholagogue action. By cholagogue I mean a something that excites the biliary function of the liver to special increased activity, and causes that larger quantity of bile, thus produced, to be discharged from the system. If, therefore, it were certain that the use of mercurials was followed, constantly, by the passing away, per anum, of considerable quantities of bile, it could not be received as proof that the biliary liver was excited to increased activity, until it was demonstrated that the amount of bile so dejected exceeded the amount normally secreted by the liver for the time involved. If mercury finds bile in the bowels and sends it on and out of them, it acts only as a sort of intestinal scavenger, and by that act no more creates the bile it removes than the street scavenger creates the bones and rags and dirt that he scrapes from the gutter and removes. The daily amount of bile normally passed into the duodenum may be roughly stated to be about equal in weight to the liver that secretes it, say, by measure, three to four pints. We have already seen that this disappears before reaching the rectum; but suppose by the use of mercurials we should succeed in causing the whole of this to be evacuated by the anus—a quantity equal in itself to ten times the whole amount of normal fæces—it would not certify the action of a cholagogue; it would not proclaim that the bile which was found in the bowel was other than the ordinary physiological product of the liver; it would only testify that such contents of the intestinal canal had been changed from a recrementitious to an excrementitious substance.

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In order, then, to demonstrate mercury a cholagogue, you must not only be assured of the excretion of the bile from the bowels, but you must also show, by direct experiment or clear induction, that this self-same bile was manufactured by the liver at the instigation of the mercury, at a time when such bile would not otherwise have been secreted. If it be said that these conditions are onerous and exacting, and render it exceedingly difficult to establish the point, I answer that such are the only terms upon which science accepts investigation and adopts conclusions. It is to the credence given to the positive declarations of prominent men, without proof; to the acceptance of the broadest generalities and the loosest deduction, without test or scrutiny; and to the unfortunate adoption of the bad logic set forth in the phrase "*post hoc ergo propter hoc*," that we find so many fallacies, not only in the theory of medicine, but in its practice also. When we shall have applied to medical studies the rigorous rules that obtain in the investigation of other inexact sciences; and consent, as a profession, not to deny before the world our ignorance in such matters as we have not been able to gain a correct knowledge of; we shall stand better with intelligent people, and give them a criterion whereby they can separate us from the vast herd of charlatans and empirics of every cast—a work at present beset with difficulties, and not always accomplished.

The belief that mercury is a cholagogue has heretofore been so general, that any one seeking the foundation upon which this belief rests, will be surprised to find how much of it consists only of the repeated asseverations of our ancestors and cotemporaries who have treated of the subject. Reliable experiments, tending to sustain the popular view, are difficult, if not impossible, to find. But the absence of testimony has a negative value, and I shall proceed to adduce it.

Stillé (Ther. and Mat. Med., 2d ed., Vol. 2, p. 789,) writes: "The power of mercury to increase bile cannot well be questioned, although the mode in which it operates upon the liver is still open to discussion." Yet he does not refer to a single author, fact or experiment to sustain his proposition. Now when so erudite a man as Stillé can give no authority for such an opinion, we may rest assured that the authority is hard to find; for his excellent book is a compilation, and his value as a writer consists in his vast bibliographical research, and his abundant and critical notation of authors.

In acute poisoning by mercury the learned Pereira (Mat. Med. and Ther. 3d Am. ed., vol. 1, p. 773,) enumerates the many symptoms that present themselves, and by examination after death finds the essential lesion to be gastro-enteritis—no mention being made of any effect on the liver. As instances of mercurial poisoning, Pereira (Ibid, p. 798,) refers to the vessels Triumph and Phipps, on board of which two hundred persons were poisoned, and two died; stating various effects of the poison, but nothing is mentioned that implicates the liver. And, again, when speaking of the physiological

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effects of calomel, he asserts that "Wepfer, Viborg, Florman, Gaspard, and Annesly, have examined the effects of calomel on dogs, horses, and pigs, but without any remarkable results," nothing relating to the liver being even mentioned. And yet he alleges, in another place, that small doses of mercurials bring in the stools "a larger proportion of bile"—an additional instance of the persistence of a preformed conclusion in the face of facts so forcibly presented by himself, not only such as here recounted, but also others, as we shall see further on.

Taylor (Med. Juris., Lond. ed.,) recounts the symptoms of poisoning by mercury, before death, and the morbid appearances after death, neither evidencing any lesion of the liver; but on the contrary, he speaks of the liver presenting no morbid appearance on examination, after death by poisoning with several of the salts of mercury.

Stillé (Ther. and Mat. Med., 2d ed., vol. 2, p. 784,) alludes to the poisoning on board the Triumph and Phipps, and also to the poisonous influence of mercurial vapors on people who live near the smelting furnaces; and the only effect on the biliary organs, that he speaks of, is that "the workmen are subject to swelling and congestion of the liver." He also says, (p. 828,) in speaking of the "defective secretion" of the liver, that "mercury is not proved to exert a favorable influence" in such condition.

Dr. Hanfield Jones' experiments, (Rank. Ab., No. 16, p. 93,) likewise, establish that the administration of mercury congests the liver. Chambers (Digest. and its Derang., p. 143,) says, upon the authority of Jones, that "mercury has also caused a very great sanguineous congestion of the liver."

So far, then, as these writers certify, it is quite clear that in acute or chronic poisoning, even unto death, by mercurials, they have no effect upon the liver, discernable in post mortem examinations; and that these drugs, instead of increasing the functional activity of the biliary apparatus, as ordinarily administered, actually produce a sanguineous congestion, and decrease secretion.

How is it about the presence of bile in mercurial stools? I will answer.

Pereira (Mat. Med. and Ther., 3d Am. ed., p. 771,) tells us that in mercurial purging, "the evacuations are frothy, whitish, tough, and often greenish, * * * from intermixed bile," the color being the only evidence of bile; and in allusion to the assertion that mercury has a specific action on the liver, increasing its biliary function and correcting its derangements, he further says, "I confess I am not acquainted with any facts warranting this broad assertion."

Stillé (Ther. and Mat. Med., 2d ed., vol. 2, p. 788,) remarks: "The green color of the stools after mercurial purges, is generally attributed to the presence of bile; and the correctness of this opinion has been demonstrated by Michéa." He refers for Michéa's demonstration to the "Monthly Retrospect" for December, 1848. It would be pleasant to examine this demonstration

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and learn in what it consists, as it is the only one of the kind, but I have not been able to find where, when, or by whom, this "Monthly Retrospect" was published. It is a significant fact, however, that Stille, in his vast research, has found but one experimentor to establish this fact.

Lehmann (Chem. Phys., p. 172,) declares that the use of calomel increases the water of the bile, but not the solid constituents.

Armor, (N. Amer. Med. Chi. Rev. Sep. 1859, p. 865,) after examining authorities in relation to calomel stools, writes: "And the facts clearly indicate that if bile be present in such discharges, it is in diminished rather than in increased quantity."

Mosler (Lond. Lancet, Oct. 27, 1860,) proved that no increase of bile followed the use of calomel, when administered in doses varying from twenty-two to fifty-two grains.

Nassé, Kölliker and Müller (Ibid,) found that the addition of calomel to food, which under ordinary circumstances produced a certain and normal quantity of bile in dogs, diminished the quantity of bile.

Thudichum, (Lond. Lan., Oct. 27, 1860,) after reviewing the results of divers experimentors, announces that "in nine cases out of ten, calomel, whether it purges or not, will diminish the quantity of bile secreted. Where it acts as a purgative, the diminution is most conspicuous."

Scott experimented on a dog by the exhibition of varying doses of calomel, and Ranking, (Abstract No. 29, p. 283,) in speaking of it, says, "the result of four experiments, conducted with great ability and extreme care, is, that there is a *diminution in the amount of fluid bile and bile-solids secreted after the administration of calomel.*" In three of Scott's four cases, the bile-acids were also largely diminished, and in one case these acids were increased.

Dalton, (Hum. Phys., 2d ed., p. 175,) after detailing the experiments on dogs whereby he arrives at the conclusion, thus announces it:

"The small intestine consequently contains, at all times, substances giving all the reactions of biliary ingredients; while in the contents of the large intestine no such substances can be recognized by Pettenkofer's test.

"The biliary matters, therefore, disappear in their passage through the intestine."

From this narrative it appears not only that there is no bile in mercurial stools, but also, that in the more satisfactory experiments, where the bile escapes through a fistulous opening, there is, instead of an increase, as asserted, an actual diminution of the secretion while the animal is under the influence of mercury.

We will now study the color of mercurial stools, the last point in our present inquiries.

The administration of mercurials is frequently followed by colored, and particularly by green, fæces. This coloration of the stools has been the

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stumbling block, from time immemorial, over which the profession blundered, and which has turned them from the path that led to correct conclusions. It was assumed that the green color was due to the presence of bile, brought there by the cholagogue action of the mercurial; an assumption, says Thudichum, "which appears to me to be at the bottom of the entire tissue of errors."

We have already seen that the physiological color of fæces is derived from various sources—from the food we eat; from the fluid we drink; from the normal secretion of the follicles of the large intestine; and from the ingestion of various alimentary and organic substances—and that pathological fæces are also variously colored, by the chemical reactions in the intestinal calibre; by the production of certain pigments from the blood; by the effusion of blood itself; and by the coloring agents that tint the leucorrheal discharge, the bronchorrheal discharge, and the discharge from the stomach in yellow fever. This has prepared us to understand how the alvine evacuations, following the exhibition of mercurials, might have other sources of coloration than the bile, for they are subject to all the influences I have recited, both physiological and pathological. Accordingly we find that many excellent men have sought for the bile in these stools and have not discovered it. These facts are sufficient to warrant the denial of any agency in the bile in giving color to mercurial fæces; but I have some testimony to call up, not only showing this, but going directly to convince of what the heightened color of these discharges is dependent upon.

I quote again from Pereira: (Mat. Med. and Ther., vol. 1, p. 799,) "The green stools which follow the administration of calomel to children, are usually supposed to arise from the action of this medicine on the liver: though Zeller thinks it depends on alteration produced in the condition of the blood; and Kraus is disposed to refer it to the operation of calomel on the milk contained in the alimentary canal. But the same colored stools are frequently observed when no mercury has been used, and there does not appear to me to be any just grounds for ascribing them to the calomel. * * The so-called calomel stools have been analyzed by Simon and Golding Bird; the results furnish no evidence of the supposed calomel origin of the stools."

Inman, (Trans. Med. Soc. St. N. Y., 1864, p. 276,) speaking on this point, says that he has sought in vain for any proof that this change of color is due to the presence of bile; neither has he been able to find any analysis of calomel stools in support of this assumption.

What, then, is it that gives the green color to the evacuations from the bowels after the administration of mercurials?

Lehmann (Chem. Phys., p. 181,) says "the grass-green stools after the use of calomel are well known; we find in them sulphuret of mercury, from which the color may in part arise by its fine subdivision."

"Hermann (N. Am. Med. Chi. Rev., Sept. 1859, p. 864,) asserts that pow-

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dered calomel, when triturated with yellowish-brown excrements, causes them to assume a greenish color."

Thudichum (Lond. Lancet, Oct. 27, 1860,) declares that "the green color of calomel stools is due to sub-sulphide of mercury, just as the black color of stools following the use of preparations of iron, is due to the sub-sulphide of iron. The sub-sulphide of mercury may be easily obtained from these stools by levigation or chemical proceedings. So much is proved."

Here ends the testimony I have to offer against the commonly accepted idea that mercury is a direct cholagogue. I am more and more convinced that the affirmative of this position is not tenable. One cannot conceive that an unprejudiced inquirer after the truth can go over the recorded evidence in regard to this subject, carefully weighing it all, and comparing one part with another, without coming to the conclusion that there is no unrebutted evidence extant that mercury has any cholagogue properties whatever—without being convinced that the idea that mercurials of any kind have cholagogue powers, has no more substantial ground than its venerableness to sustain it, to-day, among the profession.

For myself, I cannot more appropriately present my existing views on this point than by repeating what I said on a former occasion, (Trans. Ind. St. Med., Soc. 1865,) viz: "It is one of the marvels of medical science that the cholagogue property of certain drugs should have been so positively iterated, and reiterated, for hundreds of years, by all authors who had occasion to speak of them; and that the declaration should be accepted as truth by every disciple of Esculapius; and yet the proposition never had a single fact to sustain it, nor an argument, except such as that so tersely stated by Wood, in the U. S. Dispensatory, who says, when speaking of the change of color in alvine discharges, under the use of mercurials, that such change of color is 'a certain proof that the remedy is stimulating the liver, and promoting the secretion of bile;' just as if there were no possibility of giving color to the contents of the bowels, except by a pigment furnished by the liver."

The conviction then forced upon my mind by my course of study, has only been made more profound by subsequent, and a more extended, investigation.

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A CONTRIBUTION TO THE STATISTICS

IN RELATION TO

FOREIGN BODIES IN THE AIR PASSAGES.

BY J. R. WEIST, M. D., RICHMOND, IND.

A physician can scarcely be placed in a more embarrassing position than he is when hastily summoned to a little patient who is suffering in consequence of the lodgment of a foreign body in the air passages, unless he has previously determined upon his course of procedure in such a case. He finds, perhaps, a child who, a few hours before, while in the midst of play, and in the enjoyment of perfect health, was suddenly seized with symptoms of suffocation. His patient is gasping for breath, coughing violently, and looking wildly about him; his face is livid, and his eyes protrude from their sockets; froth issues from his mouth and nose, while he utters the most distressing cries. He is every moment threatened with one of the most terrible forms of death. The friends of the patient implore the medical man to do something to avert the fatal termination that seems to be so surely and swiftly approaching. All his sympathies are aroused in behalf of the sufferer, and in such a situation he will be indeed fortunate if, from previous investigation, he is able to decide promptly as to the proper treatment in the case.

One practitioner will see a number of cases in which the offending body, after producing the most alarming symptoms, will be spontaneously expelled, followed by speedy recovery. In others it will remain for months, perhaps for years, without giving the patient serious inconvenience. His own experience, therefore, leads him to the conclusion that he may safely leave all such patients to the care of nature. Another always operates upon his patients, and success follows. He is sure that bronchotomy is imperatively demanded whenever there is a foreign body in the air passages, and that the operation is attended with but little danger. Another operates, and loses all

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his patients. His experience leads him to condemn operative interference. Individual experience, then, in relation to this, as to many other questions in medicine and surgery, leads to very diverse conclusions. The truth is, that individual experience, taken alone, is of but little value; it is only by collective experience that great principles in medicine are established. "Numerical analysis in medicine is analagous to quantitative analysis in chemistry;" and in the former, as in the latter, it is only when we seek earnestly and conscientiously for the truth, "when we take the trouble, by means of weight and measure, to fix our observations, and express them in the form of equations," that we obtain correct answers to our questions.

As all facts, then, in relation to cases of a similar kind, will be of value to the practitioner in determining upon the proper treatment for his patient, when placed in the trying situation indicated, I, as a slight contribution to the statistics relating to this subject, have collected, arranged and analyzed the cases presented in the following tables.

Prior to the publication, in 1854, of Prof. Gross' valuable Treatise on Foreign Bodies in the Air Passages, no systematic work on this subject had been published; while in works on surgery it had never received the attention which its importance demanded. Prof. Gross, however, in the work referred to, "brought order out of chaos," systematized and presented the subject in a tangible and connected form.

In this work, eighty-five cases of foreign bodies in the air passages are tabulated, in which bronchotomy was not practiced. The extraneous body was not expelled, in twenty-one of these cases, or twenty-five per cent.

Of the sixty-four cases in which the foreign body was expelled, eight died, or thirteen per cent.

Of these eighty-five cases without operation, twenty-nine died, or thirty-four per cent.

A tabular list of sixty-eight cases of tracheotomy practiced for the removal of foreign bodies is given. Eight died, or twelve per cent. Of thirteen cases of laryngo-tracheotomy, three died, or twenty-three per cent. Of seventeen cases of laryngotomy, four died, or twenty-four per cent.; giving, in the ninety-eight cases operated upon, a mortality of fifteen, or fifteen per cent.; a percentage of deaths a little less than half as great as in the cases without operation.

The tables contained in this paper give the details in relation to one hundred and sixty-three cases. Of these, thirty-four have been collected from various medical journals published since the work of Prof. Gross, in 1854; while the remaining one hundred and twenty-nine cases have been collected by myself from the medical gentlemen whose names appear in connection with the cases presented in the several tables. These cases are now, for the first time, made public; and it is believed that no paper hitherto published on this subject has contained so large a number of unpublished cases.

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In the following table sixty-one cases are reported, in which recovery followed the expulsion of the foreign body without bronchotomy. Of these cases thirty-four were males and twenty-seven females. One was under one year of age; two under two; eighteen under five; eighteen under ten; nine under fifteen; and four under twenty; the remainder were, respectively, twenty-four, twenty-three, twenty-six, thirty, forty and sixty; the age in three not being specified.

The following table shows the nature of the foreign body:

Bullet	1	Sewing needle	1
Carpet-tack	2	Piece of egg shell	1
Coins	5	Piece of chestnut shell	1
Grain of coffee	3	Artificial tooth	1
Piece of bone	3	Brass clock-wheel	1
Piece of pipe-stem	1	Crumb of bread	1
Watermelon seed	12	Piece of hickory nut shell	1
Grain of corn	10	Piece of acorn	1
Bean	3	Common pin	1
Persimmon seed	1	Cockle bur	1
Piece of cabbage stalk	1	Small button	1
Pea	1	Cherry seed	1
Nail	1	Fish bone	1
Piece of slate pencil	1	Glass bead	1
Bicuspid tooth	1	Burdock bur	1

The length of time the foreign body was retained varied (if we except one case) from two hours to two years. In the exceptional case, two watermelon seeds were lodged in the trachea; one remained three and the other five years.

The method of expulsion is stated in all but two of the cases. By coughing, forty; by coughing while the head was lowered or body inverted, five; while vomiting, two; while sneezing, one; by violent expiratory effort while lifting, one; by the aid of forceps, three; with forceps by aid of laryngoscope, two; with loop of wire, one; with probang, three; with probang armed with threads of silk (in case of brass clock-wheel,) one. In one of the cases tracheotomy was attempted two weeks after the accident, but was abandoned before opening the trachea because of hemorrhage. The foreign body—a grain of corn—was expelled by coughing three months afterwards.

It will be noticed that several cases are reported in this table that do not strictly come under the head of "spontaneous expulsion," the foreign body having been removed by forceps, probangs, etc. They are placed under this head in order to contrast the cases in which bronchotomy was performed with those not subjected to this operation.

Prof. Gross' tables do not contain a single instance in which a bean was spontaneously expelled. Three such cases are here reported.

Three cases reported in the table, detailing cases of tracheotomy, in which the offending body was coughed up after the closure of the wound, might with propriety be reported under this head, as they are strictly cases of spontaneous expulsion.

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Table of Cases of Spontaneous Expulsion followed by Recovery.

NO.	AGE.	SEX.	FOREIGN BODY.	PERIOD AND MODE OF EXPULSION.	AUTHORITY.
1	9 yrs.	Male.	Bullet, ¼ inch in diameter.	3 days, during fit of coughing.	Dr. S. S. Emison, Am. Jour. Med. Sci., Oct. 1856, p. 576.
2	10 yrs.	Female.	Carpet tack, ¾ inch long.	19 days, by coughing.	Dr. Jas. Crawford, Am. Jour. Med. Sci., Jan. 1855, p. 246.
3	17 yrs.	Male.	Fourpence, ⅝ inch in diameter.	50 hours, by coughing.	Dr. S. G. Greghegan, Dublin Med. Press., Oct. 11, 1854.
4	18 mo.	Male.	Grain of coffee.	3 months and ten days, by coughing.	Dr. A. Fetherolf, Med. & Surg. Reporter, Phil., April 6. 1867.
5	12 yrs.	Female.	Piece of bone, ¾ inch long and very thin.	3 days; removed with forceps, by aid of the laryngoscope.	Mackenzie on the Use of the Laryngoscope, p. 138.
6	7 yrs.	Male.	Piece of pipe-stem, 1 inch long.	Removed from right bronchus by loop of wire.	Dr. M. Adler; Ranking's Abs., No. 33, 1861, p. 168.
7	6 yrs.	Female.	Watermelon seed.	3 weeks, by coughing.	Dr. R. E. Haughton, Richmond, Ind.
8	5 yrs.	Male.	Grain of corn.	5 days, by coughing.	
9	10 yrs.	Female.	Lima bean.	5 or 6 days, by coughing.	Dr. W. H. Scobey, Hamilton, Ohio.
10	14 yrs.	Male.	Persimmon seed.	More than a year.	Dr. W. H. Scobey, Hamilton, Ohio.
11	2 yrs.	Female.	Piece of cabbage stalk.	2 days, by coughing.	Dr. M. W. Hobbs, Richmond, Ind.
12	3 yrs.	Female.	Grain of coffee.	136 days, ejected while sneezing.	Dr. S. Ferris, Newcastle, Ind.
13	6 yrs.	Male.	Watermelon seed.	10 days, by coughing.	Dr. A. Dunlap, Springfield, Ohio.
14	60 yrs.	Male.	Gold dollar.	8 months, by coughing.	Dr. S. B. Harriman, Richmond, Ind.
15	6 yrs.	Male.	Green pea.	6 weeks, by coughing.	Dr. S. B. Harriman, Richmond, Ind.
16	Boy.	Male.	Common tack.	5 or 6 days, by coughing.	Dr. N. B. Wells, Lagrange, Ky.
17	4 yrs.	Male.	Watermelon seed.	5 weeks, by coughing.	Dr. M. H. Harding, Lawrenceburg, Ind.
18	5 yrs.	Male.	Eight-penny nail.	2 years.	Dr. Wm. F. Harvey, Plainfield, Ind.
19	5 yrs.	Female.	Large grain of corn.	9 days, standing patient on head while coughing, and striking the chest, before and behind, with the hands.	Dr. W. Griswold, Circleville, Ohio.
20	3 yrs.	Male.	Piece of egg shell (in larynx.)	10 days, by coughing.	Dr. A. G. Walter, Pittsburg, Penn.
21	26 yrs.	Male.	Three cent piece.	11 days, by coughing.	
22	12 yrs.	Male.	¼ of a chestnut shell (in larynx.)	5 weeks, by coughing.	
23	18 yrs.	Female.	Artificial tooth, broken from plate during a fit of hysteria.	7 hours, by coughing, with feet elevated and head lowered.	Dr. J. W. Martin, Lebanon, Ind.
24	4½ yrs.	Male.	Brass clock-wheel.	12 hours, removed with probang armed with several strands of silk.	Dr. A. E. Jenner, Crestline, Ohio.
25	11 yrs.	Male.	Crumb of bread.	8 days, by coughing,	Dr. L. P. Hess, Marion, Ind.
26	10 yrs.	Male.	Watermelon seed.	3 months, by coughing.	Dr. David Prince, Jacksonville, Ill.
27	12 yrs.	Male.	Piece of hickory nut shell.	10 days, by coughing.	Dr. David Prince, Jacksonville, Ill.
28	3 yrs.	Female.	Bean.	2 weeks, by coughing.	Dr. James F. Hibberd, Richmond, Ind.
29	4 yrs.	Female.	Piece of acorn.	16 days, by coughing.	Author.
30	40 yrs.	Female.	Common pin.	2 days, by coughing.	Dr. J. T. Floyd, Noblesville, Ind.
31	6 yrs.	Female.	Grain of corn.	3 weeks, by coughing.	Dr. N. Field, Jeffersonville, Ind.

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Cases of Spontaneous Expulsion followed by Recovery—Continued.

NO.	AGE.	SEX.	FOREIGN BODY.	PERIOD AND MODE OF EXPULSION.	AUTHORITY.
32	23 yrs.	Male.	Two watermelon seeds.	But little difficulty experienced after first week. Both expelled by coughing, one at the end of the third, and the other at the end of the fifth year. The seeds could be frequently heard to pass up and down the trachea.	Dr. A. G. Preston, Greencastle, Ind.
33	3 mo.	Male.	Grain of corn.	20 hours, expelled while vomiting.	Dr. J. Cochran, Spiceland, Ind.
34	9 yrs.	Male.	Silver dime.	30 hours, by coughing while suspended by the feet.	Dr. H. C. Cole, Kokomo, Ind.
35	18 yrs.	Female.	Cockle bur.	1 year, by coughing.	Dr. J. W. Moorman, Hardinsburg, Ky.
36	6 yrs.	Female.	Watermelon seed.	5 days, by coughing.	Dr. J. W. Moorman, Hardinsburg, Ky.
37	3 yrs.	Male.	Grain of corn.	24 hours, extracted with forceps.	Dr. J. W. Moorman, Hardinsburg, Ky.
38	5 yrs.	Male.	Small button.	36 hours, by coughing.	Dr. J. W. Moorman, Hardinsburg, Ky.
39	7 yrs.	Female.	Watermelon seed.	4½ months, by strong expiration while lifting.	Dr. John Lewis, Ogden, Ind.
40	8 yrs.	Male.	Grain of corn.	On tenth day, by coughing.	Dr. Israel Tennis, Richmond, Ind.
41	6 yrs.	Male.	Grain of parched corn.	End of two weeks, by coughing.	Dr. Israel Tennis, Richmond, Ind.
42	12 yrs.	Male.	Watermelon seed.	Third day, by coughing while head was lowered.	Dr. Israel Tennis, Richmond, Ind.
43	3 yrs.	Female.	Grain of roasted coffee.	In one week, by coughing.	Dr. Israel Tennis, Richmond, Ind.
44	3 yrs.	Female.	Watermelon seed.	About one year, by coughing; seed in perfect state of integrity.	Dr. V. Kersey, Richmond, Ind.
45	5 yrs.	Female.	Watermelon seed.	Over two years, by coughing; seed in perfect state of preservation.	Dr. V. Kersey, Richmond, Ind.
46	12 yrs.	Female.	Watermelon seed.	Six hours, by coughing.	Dr. Elias Fisher, Richmond, Ind.
47	14 yrs	Female.	Cherry seed.	Four hours, by coughing while vomiting from the effect of an emetic.	Dr. Elias Fisher, Richmond, Ind.
48	3 yrs.	Female.	Fish bone in larynx.	Removed in ten hours, from vocal cords.	Dr. K. G. Thomas, Alliance, Ohio.
49	30 yrs.	Male.	Piece of bone.	End of two months, by coughing; lodged in left bronchial tube.	Dr. J. Carson, Middletown, Ohio.
50	5 yrs.	Female.	Three cent piece.	In seven days, by coughing.	Dr. J. Carson, Middletown, Ohio.
51	4 yrs.	Female.	Piece of slate pencil ¾ inch long, and nearly two lines in diameter.	Sixth day, removed by probang, armed with cone-shaped piece of sponge. Inversion of body had been tried without success.	Dr. R. R. Livingston, Plattsmouth, Neb.
52	6 yrs.	Male.	Glass bead.	In a few days, by coughing while body was inverted.	Dr. P. W. Ellsworth, Hartford, Conn.
53		Female.	Bicuspid tooth; fell into the trachea while having several teeth extracted while und'r influence of Nit. Oxide gas.	Several months, by coughing.	Dr. P. W. Ellsworth, Hartford, Conn.

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Cases of Spontaneous Expulsion followed by Recovery—Continued.

NO.	AGE.	SEX.	FOREIGN BODY.	PERIOD AND MODE OF EXPULSION.	AUTHORITY.
54	4 yrs.	Male.	Grain of corn.	At end of three months, by coughing. Two weeks after accident tracheotomy was attempted, but abandoned before opening trachea, because of hemorrhage.	Dr. J. Pennington, Milton, Ind.
55	6 yrs.	Female.	Burdock bur.	End of two weeks, by coughing.	Dr. J. Pennington, Milton, Ind.
56	8 yrs.	Female.	Bean.	2 months, by coughing.	Dr. J. Pennington, Milton, Ind.
57	12 yrs.	Male.	Grain of corn.	3 months, by coughing.	Dr. J. Pennington, Milton, Ind.
58	10 yrs.	Male.	Watermelon seed.	End of six months, by coughing. Seed could be heard passing up and down the trachea during respiration.	Dr. L. J. Francisco, Richmond, Ind.
59	24 yrs.	Male.	Fish bone; lodged between vocal cords.	End of two days, with forceps by the aid of laryngoscope.	Author.
60	8 yrs.	Female.	Comm'n sewi'g needle.	Time not stated, with probang wrapped with flax.	Dr. A. E. Jenner, Crestline, Ohio.
61	Man.	Male.	Piece of bone two inches long.	About three years, by coughing.	Dr. E. Fisher, Richmond, Ind.

The following table contains a report of twenty cases, without operation, and without the expulsion of the foreign body, followed by death. Eight of the cases were males, and twelve females. One was under one year of age; four under two; ten under five, and four under ten. In one the age was forty-four.

The foreign body was, in five cases, a grain of corn; in three, a bean; in one, both a bean and grain of corn. In the other cases the offending substance was, respectively, a watermelon seed, piece of hickory nut shell, bone pencil top, a honey-locust bean, piece of acorn, piece of turnip, piece of walnut kernel, apple seed, cockle bur, pea, and a piece of walnut shell.

The time of death varied from fifteen minutes to sixteen hours after the accident, in six cases; from five to ten days, in four; from two to six weeks, in four; from six weeks to five months, in four. In one case death did not occur until the end of six years. In one the period is not stated.

In thirteen cases the cause of death is stated to have been suffocation; in two exhaustion; in one bronchitis. In four the cause is not stated.

In one of the cases an effort was made to cause the expulsion of the foreign body by inverting the body of the patient, when the offending substance —a piece of walnut shell—fell into the glottis, causing death in two minutes.

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Table of Cases of Death without Operation, and without the Expulsion of the Foreign Body.

No.	AGE.	SEX.	FOREIGN BODY.	PERIOD OF DEATH.	CAUSE OF DEATH.	AUTHORITY.
1	14 mo.	Female.	Watermelon seed.	Fourth week.	Suffocation.	Dr. John Rea, Newcastle, Ind.
2	2 yrs.	Female.	Grain of corn.	Tenth day.	Bronchitis.	Dr. N. L. Wickersham, Anderson, Ind.
3	3 yrs.	Male.	Bean.	In three months	Suffocation, by bean lodging in rima glottidis, while coughing during a fit of anger.	Dr. C. Richmond, Kokomo, Ind.
4	4 yrs.	Male.	Piece of hickory nut shell.	In eight hours.	Suffocation.	Dr. E. H. Crippen, Moscow, Ind.
5	6 yrs.	Female.	Bone pencil top.	In ¼ of an hour.	Suffocation.	Dr. Charles C. Hildreth. Reported by Dr. Z. C. McElroy, Zanesville, O.
6	3 yrs.	Female.	Bean honey-locust.	End of fifth month.	Not stated.	Dr. M. H. Harding, Lawrenceburg, Ind.
7	2½ yrs.	Female.	Grain of corn and bean.	At end of four months.	Not stated. Child seemed well, after first week, until one day before death.	Dr. W. F. Harvey, Plainfield, Ind.
8	2½ yrs.	Female.	One-half of an acorn.	Fourteenth day.	Not stated.	Dr. A. E. Jenner, Crestline, O.
9	18 mo.	Male.	Bean.	End of 42 days.	Exhaustion.	do do
10	4 yrs.	Male.	Grain of corn.	In two weeks.	Suffocation.	Dr. W. K. Mavity, Kokomo, Ind.
11	8 mo.	Female.	Grain of corn.	In five hours.	Suffocation.	Dr. L. P. Hess, Marion, Ind.
12	6 yrs.	Female.	Grain of corn.	In two weeks.	Not stated.	Dr. David Prince, Jacksonville, Ill.
13	3 yrs.	Female.	Piece of turnip.	In six hours.	Suffocation.	Dr. Israel Tennis, Richmond, Ind.
14	1 yr.	Male.	Piece of walnut kernel, size of grain of corn.	In four hours.	Suffocation. Foreign body impacted in left bronchial tube. Right lung in state of red-hepatization from pneumonia.	Dr. V. Kersey, Richmond, Ind.
15	44 yrs.	Male.	Apple seed.	End of six years	Exhaustion.	Dr. K. G. Thomas, Alliance, O.
16	6 yrs.	Female.	Bean.	End of six days.	Suffocation. (Operation refused.)	Dr. M. Sexton, Rushville, Ind.
17	8 yrs.	Male.	Cockle bur.	In 16 hours.	Suffocation.	Dr. G. W. Zimmerman, Wheeling, Ind.
18	3 yrs.	Female.	Pea.	In six days.	Suffocation.	Dr. W. H. Myers, Ft. Wayne, Ind.
19	4 yrs.	Female.	Small grain of corn.	Fifth day.	Suffocation. (Grain of corn found in glottis after death.)	do do
20	5 yrs.	Male.	Piece of walnut shell.	Not stated.	Suffocation. (Effort made to cause expulsion by inversion of body, when foreign body fell into glottis, causing death in two minutes.)	Dr. P W. Ellsworth, Hartford, Conn.

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The following table contains a list of forty-eight cases in which Tracheotomy was followed by the expulsion of the foreign body and the recovery of the patient. Twenty-eight of the cases were males, and fifteen females. In five the sex is not stated. Two of the patients were under one year of age; two under five; five under fourteen; ten under seventeen; and six under twenty. The others were aged respectively twenty-five, thirty, fifty-four, and sixty, except one in which the age is not given.

The foreign bodies were,

Grain of corn	7	Glass bead	1
Bean	6	Piece of quill	1
Grain of coffee	4	Green haw berry	1
Prune stone	3	Tamarind stone	1
Watermelon seed	2	Copper cent	1
Cockle bur	2	Cherry seed	1
Piece of egg shell	2	Brass whistle	1
Piece of chestnut shell	2	Piece of cartilage	1
Persimmon seed	2	Honey-locust seed	1
Portion of Tracheotomy tube	1	Piece of fish bone	1
Piece of pea-nut shell	1	Set from finger ring	1
Pebble	1	Small watch wheel	1
Piece of bone	1	Paw-paw seed	1
Piece of slate pencil	1		

The time of operation varied from four hours to twelve weeks after the accident. In eight cases the foreign body was removed by forceps; in twelve by coughing; in the others, the method of removal is not stated.

In forty-two of the cases, the foreign body was removed at the time of operation. In three cases it was coughed up after the closure of the wound, in one case at the end of three months, one "after several months," and one at the end of ten months, after the operation. In one case it was removed, by coughing, three days, in one seven days, and in one two weeks after operation. In one case part of the offending body (a piece of cartilage) was removed at the time of operation, and the remainder coughed up two months later.

Table of Cases of Tracheotomy, followed by the Expulsion of the Foreign Body and the Recovery of the Patient.

No.	AGE.	SEX.	FOREIGN BODY.	TIME OF OPERATION.	TIME AND MODE OF EXPULSION.	AUTHORITY.
1	7 yrs.	Male.	Persimmon seed	Sixth day.	Immediately.	Dr. E. Mason, Am. Med. Jour. Jan., 1860, page 122.
2		Male.	Portion of Tracheotomy tube.		Tracheotomy had been performed some years before, since when a double tube had been worn. The rim of the tube gave way and allowed the inner tube to pass into the trachea; two or three rings of the trachea were divided, and the tube removed with forceps.	Dr. Spence, Edin'h Med. Jour., Aug. 1862.

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Table of Cases of Tracheotomy, followed by the Expulsion of the Foreign Body and the Recovery of the Patient—Continued.

No.	AGE.	SEX.	FOREIGN BODY.	TIME OF OPERATION.	TIME AND MODE OF EXPULSION.	AUTHORITY.
3	5 yrs.		One-half peanut shell.	End of twelfth week.	7 days, by coughing.	Dr. A. North, Am. Med. Times, Nov. 28, 1863.
4	6 yrs.	Male.	Pebble, size of large bean.	End of third week.	Immediately, by coughing.	Dr. A. G. Walter, Amer. Jour. Med. Sci., Jan. 1863.
5	16 yrs.	Male.	Piece of chestnut shell, in larynx.	8 days after.	Immediately, by forceps.	do do
6	25 yrs.	Male.	Piece of bone ¾ in. long and 1-16 in. wide in larynx.	14 days after.	Immediately, by forceps.	do do
7	6 yrs.	Male.	Piece of slate pencil ¾ in. long in trachea.	5 days after.	Immediately.	do do
8	60 yrs.	Male.	Copper cent, in larynx.	4 weeks after.	Immediately, with forceps.	do do
9	13 mo.	Female.	Roasted coffee-bean.	12 days after.	Immediately.	do do
10	5 yrs.		Bean.	Next day.	Wound closed: three mos. after coughed up.	M. Rendu, reported in "Traite Pratique des Maladies des Nouveau-nes" by E. Bouchut, page 284.
11	7 yrs.		Bean.	5 days after.	Immediately.	Operator, Iszenard; reported as No. 10
12	4½ yrs		Prune stone.	Not stated.	At time of operation.	M. Weelhouse, operator; reported as No. 10.
13	5 yrs.	Male.	Prune stone.	Same day.	Immediately.	Cooper Forster, operator; authority as No. 10.
14	9 yrs.	Male.	Cherry seed.	5 days after.	At time of operation.	E. Bouchut, as No. 10.
15	15 yrs.	Male.	Brass whistle.	Soon after.	Wound healed: coughed up ten months after.	Dr. Aberle; reported as No. 10.
16	4 yrs.	Female.	Watermelon seed.	Third day.	Immediately, by coughing.	Op. by Dr. Butler; reported by Dr. R. E. Haughton, Richmond, Ind.
17	14 mo.	Female.	Coffee grain.	Fifth day.	3 days after operation, by coughing.	Dr. P. S. Conner, Cincinnati, O.
18	11 mo.	Female.	Coffee grain.	Sixth day.	At time of operation.	Dr. Rob. W. Denig, Columbus, O.
19	5 yrs.	Female.	Grain corn.	Second day.	At time of operation, with forceps.	Dr. C. Richmond, Kokomo, Ind.
20	16 mo.	Female.	Watermelon seed.	Seventh day.	Some months after wound closed, by coughing.	Dr. G. W. Clippinger, Indianapolis, Ind.
21	10 yrs.	Male.	Piece of egg shell.	Third week.	Immediately.	Dr. A. Dunlap, Springfield, Ohio.
22	18 yrs.	Female.	Piece of cartilage.	Fourth week.	Removed a portion at time of operation; remainder coughed up two mo'ths afterward.	do do
23	2 yrs.	Female.	Grain of corn.	Fourth day.	Immediately.	do do
24	6 mo.	Female.	Grain of corn.	One week.	Immediately.	do do
25	6 yrs.	Female.	Glass bead ¾ in. long.	One week.	Immediately.	do do
26	12 yrs.	Male.	Large bean.	12 hours.	At time of operation.	E. H. Crippen, Moscow, Ind.
27	8 yrs.	Male.	Bean.	4 weeks.	Two weeks after, by coughing; wound kept open.	Dr. A. Ball, Zanesville, Ohio.

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Table of Cases of Tracheotomy, followed by the Expulsion of the Foreign Body and the Recovery of the Patient—Continued.

No.	AGE.	SEX.	FOREIGN BODY.	TIME OF OPERATION.	TIME AND MODE OF EXPULSION.	AUTHORITY.
28	4 yrs.	Male.	Grain of corn.	4 weeks.	At time of operation.	Dr. A. Ball, Zanesville, Ohio.
29	6 yrs.	Male.	Piece chestnut shell.	4 weeks.	Immediately.	Dr. C. C. Hildreth, Zanesville, Ohio.
30	4 yrs.	Male.	Piece egg shell.	Tenth day.	"At time of operation, by passing probang through wound and pushing into mouth."	do do
31	8 yrs.	Male.	Seed of honey-locust.	Second week.	Immediately.	do do
32	10 yrs.	Female.	Piece of quill 1 in. long, used for toothpick.	Third week.	At time of operation.	do do
33	3 yrs.	Female.	Bean.	2 weeks.	Immediately.	do do
34	10 yrs.	Male.	Persimmon seed	Fifth day.	At time of operation, by coughing.	Dr. S. W. Thompson, Evansville, Ind.
35	3 yrs.	Male.	Grain of corn.	60 hours.	At time of operation, from right bronchus.	Dr. Cass, operator; reported by Dr. S. H. Kersey, New Westville, Ohio.
36	30 yrs.	Male.	Cockle bur.	Same day.	At time of operation.	Dr. G. C. Blackman, Cincinnati, Ohio.
37	6 yrs.	Female.	Lima bean.	Not stated.	Immediately.	do do
38	9 yrs.	Male.	Green haw berry.	Tenth day.	Immediately, by coughing.	Dr. W. S. Haymond, Monticello, Ind.
39	14 mo.	Male.	Piece fish bone ¾ in. long, ¼ in. wide, impacted in larynx.	Fifth day.	At time of operation, with forceps.	Dr. A. G. Walter, Pittsburg, Pa.
40	12 yrs.	Female.	One-half of set from finger ring, lodging in larynx.	Sixth day.	Immediately.	do do
41	3 yrs.	Male.	Cockle bur.	4 hours after admission.	Immediately.	Dr. J. W. Martin, Lebanon, Ind.
42	2 yrs.	Male.	Grain of coffee.	End of 2d day.	Immediately.	Dr. L. P. Hess, Marion, Ind.
43	11 yrs.	Male.	Small watch wheel.	Fifth day.	At time of operation.	Dr. S. Fisher, Greencastle, Ind.
44	10 yrs.	Male.	Paw-paw seed.	In 18 hours.	Immediately.	Dr. J. Carson, Middletown, Ohio.
45	5 yrs.	Male.	Grain of corn.	Fourth day.	Time and manner of expulsion not stated.	Dr. M. Sexton, Rushville, Ind.
46	7 yrs.		Grain of corn.	In 24 hours.	Not stated.	do do
47	54 yrs.	Male.	Plum stone.	In 20 hours.	At time of operation, with forceps.	Dr. R. R. Livingston, Plattsmouth Neb.
48	8 yrs.	Female.	Tamarind stone	In a few days.	At time of operation, (forcibly through wound.)	Dr. P. W. Ellsworth, Hartford, Conn.

TRACHEOTOMY FOLLOWED BY DEATH.

Nineteen cases are reported—twelve males and four females—the sex not being stated in three. Two were under one year of age; four under two; eight under five; and three under ten. The other two were aged, respectively, twenty-two and twenty-three years. In five of the cases the foreign body was a grain of corn; in four a bean; in two a piece of bone; and in

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the remainder, respectively, a piece of nail, cherry seed, prune stone, pebble, grain of coffee, piece of nut shell, shirt button, and a watermelon seed. The time of operation varied from soon after the accident to five weeks after. The time of death is thus stated: during the operation, one; soon after, five; in four hours, three; in three days, one; in one week, one; not stated, four. The cause of death was suffocation in six cases; pneumonia in two; gangrene of lungs in one; exhanstion in one; hemorrhage in one; œdema of glottis in one. In seven the cause of death is not stated.

Table of Cases of Tracheotomy followed by Death.

No.	AGE.	SEX.	FOREIGN BODY.	TIME OF OPERATION.	TIME OF DEATH AND CAUSE.	AUTHORITY.
1	23 yrs.	Male.	Upper half of shingle nail.	At end of 18 hours.	Seven weeks after operation. Tuberculosis. Nail found after death, in abscess in superior portion of right lung.	Dr. G. A. Kunkler, Western Lancet, 1856, p. 467.
2	2 yrs.	———	Cherry seed.	Soon after.	Fifteen days. Ulceration of the larynx and suffocation. After opening the trachea much relief was obtained; seed not found; wound entirely closed fifteen days after operation, when all the symptoms returned. The wound was reopened, and the seed sought for without success. Death soon followed. The foreign body was found in the trachea, but had evidently been lodged in the larynx.	M. Corbet, operator. Report by E. Bouchut, in Traite Pratique des Maladies des Nouveau-nes, p. 284.
3	5½ ys.	———	Part of prune stone.	Two days.	Almost immediately. Foreign body not found until after death, when it was discovered resting upon the superior border of the cricoid cartilage.	M. Weelhouse. Reported as No. 2.
4	6 mo.	Male.	Piece of mutton bone.	Same day.	Died in four hours. The bone was found after death resting upon the vocal cords, and firmly fixed in the mucous membrane.	Cooper Forster, operator. Reported as No. 2.
5	11 mo.	Female.	Piece of bone.	Soon after.	Died in four hours. Found on post-mortem "sticking in the rima glottidis."	Dr. Wilkes, Ranking's Abstract, No. 32, 1860, p. 178.

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Table of Cases of Tracheotomy followed by Death—Continued.

No.	AGE.	SEX.	FOREIGN BODY.	TIME OF OPERATION.	TIME OF DEATH AND CAUSE.	AUTHORITY.
6	3 yrs.	Female.	Piece of nut shell.	Not stated.	Died in four hours. After death found sticking in rima glottidis.	Dr. Wilkes, Ranking's Abstract, No. 32, 1860, p. 178.
7	4½ ys.	Male.	Grain of parched corn.	Sixth week.	Three weeks after operation. Foreign body found aft'r death wedged in left bronchial tube.	Dr. A. G. Walter, Am. Jour. Med. Sci., Jan., 1863, p. 56.
8	3 yrs.	———	Bean.	Third week.	Soon after. In this case there was also a coin lodged in the œsophagus.	Dr. Miner, Med. and Surg. Reporter, Phil'a, Aug. 25, 1866.
9	7 yrs.	Male.	Shirt button.	Third day.	Soon after the closure of wound; from œdema of the glottis.	Dr. R. R. Livingston, Plattsm'th, Nebraska.
10	7 yrs.	Male.	Pebble.	Twelfth day.	Died in one week. Foreign body not removed.	Dr. Fitch, operator. Reported by C. Richmond, Kokomo, Ind.
11	1½ ys.	Male.	Grain of corn.	Second week.	Died from pneumonia. Period not stated.	Dr. Charles C. Hildreth, operator. Reported by Dr. Z. C. McElroy, Zanesville, O.
12	3 yrs.	Male.	Coffee grains.	Not stated.	Died from gangrene of lungs. One grain removed at time of operation. Died six months after operation, when left lung was found gangrenous, and grain found in middle lobe.	Dr. Geo. C. Blackman, Cincinnati, O
13	3 yrs.	Male.	Bean.	One week.	Three days after operation.	Dr. W. H. Harding, Lawrenceburg, Ind.
14	3 yrs.	Male.	Grain of corn.	Thirty-fifth day.	Not stated.	Dr. A. E. Jenner, Crestline, O.
15	2 yrs.	Female.	Grain of corn.	In forty-eight hours.	Soon after operation.	Dr. J. W. Martin, Lebanon, Ind.
16	2 yrs.	Male.	Bean.	In twenty-four hours.	Asphyxia. Died soon after operation.	Dr. A. G. Preston, Greencastle, Ind.
17	22 mo.	Male.	Watermelon seed.	In twenty hours.	Soon after operation.	do do
18	5 yrs.	Female.	Bean.	Seventh day.	During operation, from hemorrhage. (Singular distribution of vessels.)	Dr. David Prince, Jacksonville, Ill.
19	4 yrs.	Male.	Grain of corn.	Not stated.	Foreign body impacted in bronchial tube; could not be removed. Died from inflammation of lungs.	Dr. M. Sexton, Rushville, Ind.

The following table contains a report of ten cases of Laryngotomy, followed by the expulsion of the foreign body and the recovery of the patients. Five of the patients were males, and five females. One was under two years of age, and four under five. One was aged twenty-three, one thirty, and one thirty-two. In two cases the age is not stated. The foreign body

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was in two cases a cockle bur, and in the others, respectively, a grain of corn; grain of coffee, piece of dried apple, pin, coin, watermelon seed, piece of egg shell, and a large-sized tailor's needle. The time of operation varied from eighteen hours to ten weeks after the accident. In all the cases the offending body was removed at the time of the operation. In one case it was expelled from the wound by coughing; in three cases it was removed with forceps; in the others the method of removal is not stated. In one case the foreign body was not found.

Table of Cases of Laryngotomy, followed by the Expulsion of the Foreign Body and the Recovery of the Patient.

No.	AGE.	SEX.	FOREIGN BODY.	TIME OF OPERATION.	TIME AND MODE OF EXPULSION.	AUTHORITY.
1	32 yrs.	Female.	Pin.	In two weeks.	Pin not found at the time of the operation, but relief and recov'ry followed.	Dr. A. North, Am. Med. Times, Nov. 28, 1863.
2	Man.	Male.	Sixpence.	In ten weeks.	At time of operation.	Dr. J. B. Sanderson, Med. Times and Gazette, June 24, 1865.
3	4 yrs.	Male.	Watermelon seed.	Eighth day.	At time of operat'n, by coughing.	Dr. N. Gay. Columbus, Ohio.
4	3 yrs.	Female.	Piece egg shell.	Second day.	At time of operat'n, with forceps.	Dr. N. Gay, Columbus, Ohio.
5	2 yrs.	Female.	Piece dried apple.	In eighteen h'rs	At time of operation.	Dr. N. Gay, Columbus, Ohio.
6	2 yrs.	Male.	Grain of corn.	Fourth day.	Expulsion at time operation, from wound, during fit of coughing.	Dr. Philip Harvey, Burlington, Iowa.
7	23 yrs.	Female.	Cockle bur.	Eighth day.	At time of operation.	Dr. Geo. C. Blackman, Cincinnati, O
8	30 yrs.	Male.	Cockle bur.	Some days after accident.	Immediately.	Dr. Geo. C. Blackman, Cincinnati, O
9	15 mo.	Female.	Part of a coffee grain.	Sixth day.	At time of operation, body strongly impacted in the larynx.	Dr. Washington L. Atlee, Philadelphia, Penn.
10		Male.	Large-siz'd tailor's needle.	Soon after.	At time of operation, with forceps.	Dr. L. B. Bastford, Lancet & Observer, 1862, page 313.

The next table gives details in relation to five cases of Laryngo-tracheotomy, followed by the expulsion of the offending substance, and the recovery of the patient. Three of the cases were males; one female; in one the sex is not stated. The age of the patient is given in four cases—one being three, one seven, one nine, and one twenty-seven years old. In four cases the operation was performed, respectively, in two, four, six and sixteen days; in one the time is not stated. In four cases the foreign body was removed at the time of operation, and in one two days after. In one case it was expelled by coughing; removed in two cases with forceps, in one by the fingers, while in one the method is not reported.

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Table of Cases of Laryngo-Tracheotomy, followed by the Expulsion of the Foreign Body and the Recovery of the Patient.

No.	AGE.	SEX.	FOREIGN BODY.	TIME OF OPERATION.	TIME AND MODE OF EXPULSION.	AUTHORITY.
1	3 yrs.	Male.	Piece of tin, 7-10 inch. long and 3-20 wide.	End of 4 days.	Two days after operation, by coughing.	Dr. Paul F. Eve, Am. Med. & Surg. Jour. July 1858, p. 290.
2	7 yrs.	Female.	Piece of hickory nut shell.	Sixteenth day.	At time of operation, with fingers, from wound.	Dr. N. Gay, Columbus, Ohio.
3	9 yrs.	Male.	Pebble.	Sixth day.	At time of operation, with forceps.	Dr. N. Gay, Columbus, Ohio.
4	27 yrs.	Male.	Piece of bone.	Second day.	At time of operation, with forceps.	Author.
5			Wheat or rye beard.		At time of operation.	Dr. G. C. Blackman, Cincinnati, Ohio.

In the preceding tables 81 cases are reported not subjected to Bronchotomy. In 20 of these the foreign body was not expelled, or 24.69 per cent., which tallies in this particular almost exactly with the number reported in the tables of Prof. Gross, which give 24.70 per cent. In other respects, however, there is a marked difference. In the tables here presented it will be observed that no cases of death occurring after spontaneous expulsion are reported, while Prof. Gross' tables show a mortality of 13 per cent. in the same class of cases. It is proper to remark here, that one case of death following spontaneous expulsion has been reported to me. This case, reported by Prof. Blackman, of Cincinnati, was one in which a puff-dart was spontaneously expelled, the patient dying some weeks after; but being unable to obtain the particulars in relation to the case, it could not be tabulated.

Sixty-seven cases of Tracheotomy give a mortality of 19, or 28.35 per cent., against a mortality of only 12 per cent. in the work above referred to.

No deaths following either laryngotomy or laryngo-tracheotomy are reported.

The 82 cases subjected to operation, give 19 deaths, or 23.17 per cent., against a mortality of 15 per cent. in the similar cases reported by Prof. Gross. There is here a wide difference, and this becomes more observable when the chances for life, with or without operative procedure, as determined by the ratio of deaths in the two tables, are compared.

As determined by Prof. Gross' tables, the chances for recovery are more than twice as great after bronchotomy as they are without this operation; while the cases here presented show only a difference of 1½ per cent. in favor of operation; and I feel sure, from observations made during the collection of material for this paper, that were it possible to collect from medical men generally *all* the facts known to them in relation to this subject, the difference in favor of operations would be reduced still more; for I have found among many medical men a disinclination to report cases where operations have been followed by fatal results, as well as in those which, though the patient

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ultimately recovered, death for a long time seemed imminent, while the practitioner waited uncertain as to what he should do. This is much to be regretted, for without all the facts, it is very difficult, if not entirely impossible, to arrive at true conclusions.

My intention was, originally, to follow these tables with a detailed report of several interesting cases, together with some observations upon the treatment in cases of this accident, and upon the value of the laryngoscope in the diagnosis and treatment of certain cases; but a want of time has prevented my carrying out my original plan, therefore the paper is offered in its present shape. Perhaps the material remaining on hand may be worked up at some future time.

In conclusion, I wish to return my thanks to the many gentlemen who have so kindly contributed the material out of which this paper has been elaborated.

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REPORT ON CHOLERA.

BY GEORGE SUTTON, M. D., AURORA, IND.

As cholera has not prevailed extensively over the State of Indiana during the summer of 1866, I present at the present session of the State Medical Society only a brief report from counties. No effort was made to collect facts except from a few localities where I heard of cholera prevailing, as I thought it better before sending out circulars to procure information, to wait the further progress of the epidemic, regarding it as highly probable that cholera would again return to our State before it entirely left the country.

In the spring of 1853 I presented to the Indiana State Medical Society a report on cholera, showing the extent to which this disease prevailed within this State during the years 1849, 1850, 1851 and 1852. The principal object I had in view in drawing up that report was the collection of facts from which we could ascertain the extent to which the disease prevailed within our State, and how far healthy or unhealthy localities retarded or favored the progress of the disease, and also the treatment generally adopted by our physicians. I received communications from forty-six physicians showing the extent to which the disease prevailed in thirty-eight counties; and the information presented by physicians in that report is the only history, that I am aware of, that has yet been published of the progress of Asiatic cholera through Indiana. As that report closed in the fall of 1852, I merely mention at the present time in connection with it, the fact that cholera again prevailed to a limited extent in Indiana in the year 1854. From notes which I took at the time, I find that the disease made its appearance in Aurora, that year, on the 26th day of June, in a family residing about twenty yards from the railroad track, near the depot. The disease continued to prevail until about the 10th of August, almost entirely confined to this locality. We had between fifty and sixty cases and twenty-two deaths. As most of the cases came under my care, I had an opportunity to observe the progress of the disease, and to examine the causes likely to favor its development; but I could not discover the manner in which it was introduced, neither could I discover any cause arising from the locality or the

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manner of living of its inhabitants that would favor the spread of the disease more in this part of the town than any other. Cholera was prevailing at this time both in Cincinnati and St. Louis, and as the first case occurred within a few feet of the Ohio and Mississippi Railroad—the great thoroughfare between these two cities—it will be considered highly probable, by those who regard the disease infectious, that it was introduced from this source; and when once introduced, the infection accumulated within this locality. But few cases occurred in the surrounding country; one, however, may be mentioned as showing the probability of its being communicated. A young man by the name of Alexander, whom I had been attending for diarrhea, left the infected district and went to his parents, who resided in a dry and healthy situation about nine miles from Aurora. In the house at which this man had been boarding, two persons had died of cholera, and at the time he left I was attending in this house a woman in the collapse stage of the disease. The next morning after his return home his mother was suddenly attacked with cholera in its worst form, and died after a few hours illness. There was no other case in that neighborhood. After cholera disappeared from our city that year we saw no more of the disease until its return in the summer of 1866.

On the reappearence of cholera in New York, in the spring of 1866, I procured, as a member of the Board of Health, the passage of an ordinance by our City Council, giving the Board full power to disinfect premises and use all means necessary to prevent the spread of the disease, should it again make its appearance in our city. As Aurora had been severely scourged with cholera in 1849, we endeavored, by the cleansing of streets and alleys, to have our city in as healthy a condition as possible, and up to the appearance of the disease our inhabitants were unusually healthy, there being no premonitory symptoms of any kind of an approaching epidemic.

On Sunday night, the 12th of August, 1866, while cholera was prevailing in Cincinnati, a Mrs. Powel, living in Aurora, was suddenly attacked with the disease, and died after about twelve hours illness. As the symptoms were clear and well marked, showing undoubtedly a case of Asiatic cholera, the Board of Health ordered the disinfection of the premises, but the friends obstinately objecting, it was not done. On Tuesday evening following, the husband of this woman was attacked with cholera, and died in the same house after a short illness. The premises were now thoroughly disinfected with sulphate of iron, and the bedding removed. The only symptoms of the disease at this house, that followed these two deaths, were a mild case of cholera morbus and a case of diarrhea, which occurred a few days afterward.

As these two deaths were the first cases of cholera that had occurred at Aurora since 1854, much interest was felt to ascertain whether we could trace the manner of its introduction into our town; as the connection between cases can generally be much more easily seen upon its first introduction into

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a place than at any other time. I was informed that Mrs. Powel had been in attendance on her aunt, a Mrs. Day, who had left Cincinnati, and was taken unwell while visiting her mother residing in the country near Lawrenceburg, and died. Mrs. Powel, the Saturday before she was attacked, washed Mrs. Day's clothing. Dr. C. B. Miller, of Lawrenceburg, the attending physician of Mrs. Day, sends me the following history of the case:

"I was called at 9 A. M., August 9th, to see Mrs. Elizabeth Day, who came from a cholera-infected district of Cincinnati, where she saw three persons who had died with the disease in the house adjoining her own a few days before. She had recently been under the treatment of a homeopathist. I found her utterly prostrated but conscious; extremities cold; no pulse perceptible at the wrist; skin bathed in a cold, clammy perspiration; with frequent copious rice-water discharges from the stomach and bowels. The eyes were dull and sunken; the tongue moist, and furred light brown; the skin, on being pinched up, retained for some time the impression made upon it by the fingers. Gave her brandy ℥ss., with sulphate of morphiæ gr. one-fifth, which she retained on her stomach. In half an hour gave hydrarg. chlor. mit. gr. j, with morphiæ sulph. gr. ⅕, and repeated the dose every two hours, alternated with brandy ℥ss. and camphor gr. v; ordered her wrapped in hot blankets, with bottles of hot water to the extremities and mustard sinapisms over the stomach. In the evening found her rather better, and, as she had vomited but little, and was still troubled with diarrhea, gave her opii gr. j, plumb. acet. gr. j, camphor. gr. v, every three hours through the night, still giving her frequent doses of brandy. On the morning of the 10th found her much the same, except the diarrhea and vomiting were not so bad, her extremities were less cold, and there was also pulse perceptible at the wrist. Continued treatment, and ordered her sponged with whisky and water. August 11th—Found her somewhat improved in every respect, though extremities remained cold and vomiting occasionally occurred, with diarrhea. There seemed to be some exacerbation every evening, and she was put upon two grains of quinine every two hours for the next twenty-four hours. Continued brandy and opiates, if necessary to control diarrhea; also hot blankets to extremities, with hot fomentations over the stomach and bowels. August 12th—Found her with a high fever, somewhat delirious, with a dry tongue heavily coated with a dark brown fur. The limbs became warm during the night and the clammy perspiration disappeared; she vomits occasionally a matter resembling curdled milk colored a bright green; discharges from the bowels about of the same character as heretofore; only four or five in the last twenty-four hours. Gave her hydrarg. c. creta gr. ij, opium gr. j, camphor gr. ij, every four hours, alternated with emulsio olei terebinth. ʒj. Directed sponging to be continued, and brandy in the absence of fever; the reaction was too great, and she sank and died during the night. The discharges from the bowels became involuntary and very copious the last hour or two;

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but she was at no time troubled with cramps of any consequence, although she frequently would seem almost suffocated and exhausted. The case was regarded as one of pernicious or congestive fever, combined with a cholera influence in the system. That there was cholera about it was proved by the fact that nearly all who attended her were more or less affected with symptoms of the disease, and some were seriously ill. I had a severe attack of diarrhea for a day or two, commencing on the second day of attendance on her. My wife was also attacked with severe diarrhea and vomiting, with cramping of the extremities, which yielded only to a very active treatment."

Mr. George Laird, a brother of Mrs. Day, furnished me additional information in relation to this case. He says that "Mrs. Powel, (the neice,) on the Saturday after Mrs. Day's death washed her clothing: and that in his mother's family there were six persons—four boys, his mother and Mrs. Day. Three of the boys were severely attacked with diarrhea, and were so unwell that they could not go to the burial of their sister. Mr. Day, the husband, also had diarrhea. In Mr. George Laird's family, there were nine persons—three boys, four girls, himself and wife. Out of this number seven were attacked with diarrhea—Mr. Laird, his wife, three girls and two boys: all had seen Mrs. Day. The two youngest escaped. Some of them were attacked very suddenly and severely. Both families were healthy previous to the arrival of Mrs. Day, and all these cases of diarrhea occurred within twelve days after her death." Mr. Laird lives in a dry and elevated situation, one of the most healthy in the whole country, so that there were no local causes to produce disease.

On Saturday, the 8th of September, Joseph Becht came from Cincinnati to visit his son-in-law, Peter Bidner, residing in Aurora. On the Monday following, this man had an attack of diarrhea, accompanied with cramps of the extremities. He soon recovered, and no importance was attached to the case. On the Thursday following, Mrs. Snider residing at Bidner's house, and Bidner's son, were both attacked with vomiting and purging: the boy died after about ten hours illness; Mrs. Snider gradually recovered. These cases, although looked upon with suspicion, were not yet regarded with certainty as being cholera by the attending physician. On the Sunday morning following, I was called in consultation to see Bidner, whom I found in the collapsed stage of cholera, and who died a few hours afterward. While attending on Bidner, I was called to see a child of Mrs. Snider, which was also in the collapse stage of cholera, and died that day. I was also hurriedly called to an adjoining house to attend a little girl that was suddenly attacked with vomiting and purging and well marked symptoms of cholera. Also during the day I was called to see three additional cases in adjoining houses, all presenting the well marked symptoms of cholera. We had in this locality, within twenty-four hours after I was first called, six

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well marked cases of cholera, and two deaths. These cases occurred in four houses, on the outskirts of the town. These houses were isolated, being almost entirely surrounded by open commons. In these four houses eight families resided, consisting of sixteen adults—seven males and nine females, and twenty children—eleven boys and nine girls. Out of this number, we had twenty-one attacked with what I considered cholera, within two weeks from the introduction of the first case. There were ten deaths—two men, one woman, six boys and one girl. There were four cases of cholera that recovered from the collapse stage of the disease; seven cases that assumed only the diarrheal form; and one presenting dysenteric symptoms. There were fifteen persons out of the thirty-six who escaped altogether, although equally exposed to the influences that were producing cholera around them.

Mrs. Dennerline, a widow, the owner of one of these houses, had nine persons in her family. Five out of the nine were attacked with the disease; two died—a son about fifteen years old and a daughter about nine years of age. She was in constant attendance upon her family, day and night, for more than a week. This I know to be a fact, as I visited them at all hours, they being under my care. I have found her at midnight, lying on the same bed with her sick children—and one of them in collapse at the same time—suffering the depressing influence of grief for the loss of two of her children and her brother, and the anxiety arising from the other members of her family being ill, and from witnessing the sudden deaths of her neighbors in another family residing in her house; yet she had no symptoms of the disease. In other rooms of this house, another family lived, consisting of four persons. Three out of the four of this family were attacked with the disease, and one died. These facts show, on a small scale, what we witness when cholera prevails as an epidemic in a large city: that some persons will be attacked with diarrhea, others with the malignant form of the disease, and others will escape; showing that all are not alike susceptible to the disease. Had all the thirty-six inhabitants of these four houses been as insusceptible to cholera as Mrs. Dennerline, the first case which introduced the disease into this locality might have been cholera in its most malignant form, and still there would have been no further appearance of the disease.

A young woman, residing in another part of the town, came to attend the sick; on her return home, she was attacked with cholera, and died after about twelve hours illness. Having full confidence in the efficacy of disinfectants, I superintended their application to all these premises, and saw that everything likely to spread the disease was carefully and thoroughly disinfected. The disease was confined to this locality; but whether this was owing to our disinfectants, or to a want of susceptibility in our community to the disease, the future progress of the epidemic will probably show. There was a slaughter house near these buildings, which probably may have had some

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influence in making the disease malignant, but there were other slaughter houses near town, near which there were no cases of cholera.

We had several cases which it is highly probable were cholera from which the disease did not spread.

August 16th, I was called about midnight to attend Mr. S., who had just returned from Louisville. He was vomiting; had profuse rice-water evacuations, cramps of the extremities, profuse perspiration, and a small and frequent pulse. The vomiting and purging were arrested in the course of a few hours, but the cramps of the extremities continued for six or eight hours longer. There were seven persons in this family, but no other had any symptoms of the disease.

August 19th, Mr. H., a commission merchant of Cincinnati, came down on a visit to his parents residing in Aurora. Cholera was at this time prevailing in Cincinnati, and a number of deaths had occurred in the vicinity of his business house. Shortly after his arrival in Aurora, he was suddenly attacked with vomiting and diarrhea. I was called to attend him, and found that his disease in a short time manifested the symptoms of cholera. There were profuse rice-water discharges, vomiting, cramps of the extremities, and a small and rapid pulse. The skin assumed a purple appearance, and at the same time was bathed in perspiration. He gradually recovered. There were six persons in this family, five of whom escaped any symptoms of the disease. The servant girl had an attack of diarrhea before Mr. H. entirely recovered. There was another case which was supposed to have been cholera, from which the disease did not spread. In all these cases, the premises were thoroughly disinfected with sulphate of iron, and the evacuations immediately removed.

Cholera also prevailed in Ripley county last summer. Dr. W. L. Graves, of Versailles, sends me the following information:

"About the last of August, 1866, a few cases of cholera occurred at Nolton, in Ripley county, but I have no history of them that can be relied upon. About the same time a man named Andrews, at Osgood, took the disease and died. His brother-in-law attended the funeral, and was about the house where the corpse lay. He went to his home, distant about four miles from Osgood, and, on the third day after the burial, took the disease and died. Several Irish people were about the house while Andrews was sick, or after his death, and of them there were three deaths by cholera. Two other persons are reported to have died of the disease, who were about the house while Andrews was sick, or soon after his death. These were the first cases occurring in the neighborhood. Cholera was prevailing at Cincinnati, and it was said some of the trains on the Ohio and Mississippi Railroad had cholera patients on board. About the same time a man was left off a train at Pierceville, after night, while there was a heavy rain. The man being sick, and the citizens having retired, he remained on the floor of the piazza of

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Mr. H. Bussell's residence until next morning. During the night he had vomited, and probably purged, several times on or about the floor. At one end of the piazza was a well or cistern from which people were accustomed to get water, and it is probable that the sick man went there for water before he became too much prostrated to attend to his own wants. He died soon after he was removed from the piazza. Two days after this man died Mr. Bussell was attacked with cholera, and died after a few hours' illness. Another citizen of the town was attacked with the disease, and died within two or three hours of Bussell's death. I am informed that several deaths occurred in the town and its immediate vicinity within a few days, and all of these persons were known to have been about Bussell's house after the first case occurred. Of these cases I can get only the general information that several were sick who did not die, and that there continued to be new and fatal cases until the people ceased to use the water supplied by the well or cistern mentioned.

"On the 16th day of September an aged maiden named Connelly, residing two miles from Versailles, was attacked severely, and died in about fifteen hours. She had previously been in good health, and had not been from home for some months. Her sister—also unmarried and aged—had been suffering with dysentery some three or four weeks, and was convalescing, but still continued to take medicine, under the direction of a physician of this place. The doctor had been visiting cases of cholera at Nolton and Poston, and generally saw Miss Connelly as he returned home from those places. On the morning of September 14th, Miss Connelly (the one who had been suffering with dysentery,) was alarmed by suddenly having three alvine evacuations of an enormous quantity of clear fluid, with nausea and prostration. She took opiates and astringents that had been prepared for her previously, and soon recovered without sending for a physician. This attack was on Friday, September 14th, at about daylight. Her sister was engaged with her all the time, and on the next day (Saturday, 15th) she washed the clothing and bed clothes that had been used in and about the sick room. At night she complained of fatigue and loss of sleep. About three o'clock A. M. of the 16th she began to vomit and have frequent movements of the bowels, and sent for a physician. He saw her about five o'clock that morning, and immediately ascertained that she had cholera. She died about nine o'clock that evening. There were no other cases in that family or neighborhood.

"Did the invalid sister have cholera? Did the physician convey the poison to these persons?

"The only case of cholera that came under my notice was that of Mr. John Hunter, aged seventy-four. He had been in Cincinnati from October 29th until November 3d, and when he returned home on the 3d of November, he was complaining of having taken cold and of some derangement of the

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bowels. For the diarrhea he was using some domestic remedies, until Sunday night, November 4th. He then asked the advice of a physician, who gave him a powder containing calomel gr. ii, pul. opium gr. i, tannin gr. iv; to be repeated in three hours. About sunrise I was called, and found Mr. Hunter suffering severely. He was vomiting a clear, serous fluid, in rather large quantities, every few minutes, and at each convulsion of vomiting the muscles of the legs and thighs were spasmodically contracted. There was precordial distress, anxious countenance, small hard pulse of 85, surface indicating retarded capillary circulation, and a tendency to sweat, but not to the same extent that usually accompanies collapse. I added two grains of gum camphor to each of the powders, and continued them until four had been retained. I also gave twenty drops of Chandler's Chlorodyne every hour until the stomach ceased to reject it; applied sinapisms to the epigastrium and frictions to the surface generally, especially to the arms and legs. About eleven o'clock A. M. the vomiting had nearly ceased, and the action of the bowels was much less frequent and not accompanied by any cramps; the pulse was a little more full and the surface less dingy or congested. From this time there was very little apparent change until about four hours before death. I neglected to state that there had been but very little urine voided since the evening of the 3d of November. Continued to give hydrarg. chlor. mit. grs. ii, pulv. opium gr. ss, camphor grs. ij, tannin grs. ij, every four hours. He had an action of the bowels about every three hours; and vomiting, slightly, every four or five hours; and an occasional spell of precordial distress, but not severe. Twenty-four hours after I first saw him I found him with a rather feeble pulse of 90; skin cool, moist, and rather clammy; tongue dark, dry, and slightly red; had had no action of the bowels for the last eight hours; vomited twice during the night; precordial distress still coming on in paroxysms; countenance rather dull; no coma. Continued the alterative powders, and gave brandy occasionally. At eight o'clock P. M. the pulse began to get variable; respiration rather hurried and laborious, with sighings and jactitation; skin became cool and clammy; countenance dull and vacant, with a dark tinge, as if the capillary circulation was obstructed; pulse about 95, weak and thready; and he died at one o'clock A. M., November 6th.

"There had been great care taken to remove from the sick chamber all articles that might be liable to become vehicles of communicating the disease. All discharges and soiled articles were immediately removed. The discharges were freely plied with the sulphate of iron, and the clothing was ordered to be immediately plunged into boiling water. The room was well ventilated. No other case occurred in the neighborhood."

Cholera also prevailed in Switzerland county, on Grant's Creek, in Posey township. The following information was furnished me by Mr. A. J. Scott, a reliable gentleman, residing in the neighborhood in which the disease ap-

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peared. Mr. Scott writes that "a man by the name of Marcus came from Cincinnati on the 18th of August, 1866, to the residence of Robert Craig, on Grant's Creek. The next day after his arrival he was severely attacked with diarrhea, which continued until the 22d, when the disease terminated fatally. On the 27th Robert Craig was attacked with vomiting, purging, and cramps of the extremities, and died on the 28th. Elizabeth Hickman, residing in the same house, was attacked the next day after Robert Craig died, with vomiting, purging and cramps. The vomiting and cramps were arrested, but the diarrhea continued, and she died September 13th. Joseph Strope attended on Robert Craig, and after his death laid him out, dressed and shaved the corpse. In seven days from Craig's death, September 5th, Strope was attacked with vomiting, purging and cramps, and died after twelve hours illness. Henry Strope, in the same family, was attacked on the 6th of September severely with diarrhea, but recovered after a few days illness. Matilda Strope, his wife, was also severely attacked with diarrhea, and also recovered."

Dr. James H. Peyton, of Rising Sun, Ohio county, one of the attending physicians in these cases, sends me the following information, which not only corroborates the statements of Mr. Scott, but furnishes some interesting facts of the prevalence of the disease in Ohio county. The doctor saw several cases resembling cholera during the early part of the summer, which were produced from overloading the stomach with indigestible food. They all recovered after a short illness. He says:

"August 22, 1866, I was called to see Abraham Kay. This man had been waiting on a man who died on the roadside about two miles below Rising Sun. The man who died and his wife were going down the river in a skiff, and landed on the Indiana side to cook dinner. While on shore some rude boys stole their skiff, and left them to make their way on foot. They traveled about two miles, and camped on the roadside under a tree. During the night the man was attacked with cholera, which soon proved fatal. Mr. Kay was the only person who administered to his wants, except his physician. Mr. Kay was soon afterward attacked with all the characteristic symptoms of cholera, and came nearer dying than any man I ever saw recover. For four days he was in a state of collapse and almost complete coma. During the entire four days his surface was cold and presented that peculiar leaden hue so characteristic of the collapse of cholera. He passed no urine during the whole time of his coma.

"The next case I saw was that of Joseph Strope, who lived on Grant's Creek, distant about eight miles from Rising Sun. This case occurred September 5, 1866. The man was in the algid stage of cholera when I reached his house, and lived only two hours after my arrival. What is remarkable in this case is, that a short time before he was attacked with cholera, a man from Cincinnati came to one of his neighbors and died of cholera. This

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neighbor, a Mr. Craig, in a day or two after was attacked with cholera and died. His wife shortly after was also attacked with cholera and died. This man Strope waited on them all and attended their burials.

"The next case that came under my notice was on September 8th. This case was a very intemperate young man who had visited Covington, Ky., and Cincinnati, only a few days before the attack came on. He was vomiting and purging, and severely cramped in the lower extremities. He had the characteristic rice-water discharges. His disease yielded readily to treatment, and his recovery was rapid."

These are the principal facts, which I have been able to collect, of the prevalence of cholera in the south-eastern portion of our State during the summer of 1866. Should the disease again return this summer, an effort will be made to collect facts from more distant portions of the State, for the benefit of the Indiana State Medical Society; and to show more fully the treatment adopted by our physicians.

In the report which I presented to this society in the spring of 1853, I advocated the idea that cholera was diffused over the globe by human agency. The evidence which I had received and the facts which I had witnessed, I thought fully sustained these views. Having had an extensive experience with cholera—witnessing its prevalence in Cincinnati in 1832, 1833, 1834 and 1835—being a student of medicine at the time, and having an opportunity of seeing the patients of my preceptors—also seeing my first preceptor die from the epidemic after only a few hours illness; and again witnessing cholera still more fatal in 1849, at which time I was prostrated from a severe attack of the disease; and while confined to the bed seeing my family stricken down, my son die, and other members of my family in the lowest stage of collapse from which the system could recover; while at the same time more than half the inhabitants in the square in which I resided (my nearest neighbors) died from cholera—with this experience I had no hesitation in advancing opinions which I thought would account for some of the obscurity that accompanies the disease. In that report I advanced the idea that cholera, like other diseases, presents different grades of severity; and that the choleraic diarrhea, which at that time was usually considered a premonitory symptom, was in reality a mild form of the disease. I divided cholera into four forms—the diarrhea form; the dysentery form; the mild form, resembling cholera morbus; and the malignant form: and considered each form capable of communicating the disease. Additional experience with cholera convinces me more fully of the correctness of these views.

In accounting for the malignancy of cholera in certain localities in the city of Aurora, I advanced the idea in that report that *the infection arose from the evacuations*; which you will find on pages 162, 163, and 166, in the following languge. On page 162: "But if we consider the disease as having prevailed in a mild form, and then, as season and other causes favored its

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development; assuming a malignant character; and, also, that the infection is capable of accumulating, and becoming concentrated in certain localities; it appears to me that we can understand the manner in which cholera prevailed in Aurora (so severely) in the infected district. Six or seven hours before the first case terminated fatally, the evacuations from the bowels passed involuntarily into the bed; consequently the bed and straw became saturated with these discharges. Immediately after the death of the patient this bed was emptied upon a vacant lot on the west side of this house. Now if we can conceive that from this straw there emanated a poison capable of producing cholera, that part of the town which became infected is just that portion which a vapor emanating from this place would be most likely to pass over. The patient died on the 5th of June, and from the 5th until the 9th the weather was calm, damp and sultry. If a small animal had been lying upon this lot in a state of putrefaction, I have no doubt but that all persons living within the infected district would have been sensible of it from the offensive effluvia passing into the atmosphere. As this straw dried there must also have been a vapor rising from it into the atmosphere; and if we view this vapor as producing cholera, we can understand how the inhabitants in front and in a northeast direction became affected; and as fresh cases occurred the different houses became foci of infection, from similar causes, and thus the disease extended along the street."

Again, on page 163, is the following language: "When the disease prevails each house at which a fatal case has occurred becomes a source of infection, first from the patient, next from the bed and bedding, also from the excretions which, from their watery appearance, are generally emptied on the ground. If the exhalations from a cholera patient, or from the discharges, were as easily detected by the sense of smell as the effluvia from carrion, the odor of musk, or particularly the stench of the skunk, we should not have the least hesitation in considering the disease as extending itself through the atmosphere."

Again, on page 166, is the following: "Our conclusions, then, are that cholera is propagated by laws peculiar to itself, but that these laws come under the head of those which govern infectious diseases. The question as to whether cholera is infectious or not is an important one in several respects. We are told in many of our best medical works that cholera is not infectious, and consequently cannot be communicated from one individual to another. If these works are wrong they may aid in extending the disease by putting physicians off their guard, preventing them from adopting precautionary means to arrest its progress, by allowing persons to remain crowded in poorly ventilated houses, where the disease has appeared; by preventing the adoption of necessary means to purify the buildings in which cases of cholera have occurred; by allowing the clothes and bedding to

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be exposed where the infection may be communicated; by permitting the excretions to be emptied upon the ground, where, by their evaporation, they may infect the atmosphere, &c., &c. These causes, when not attended to, must, in my opinion, have a tendency to spread the disease."

I again present these views to our Society, as the theory has only recently become popular that cholera infection arises from the evacuations; but it can be seen that this theory was presented to the Indiana State Medical Society fourteen years ago. It was the theory that I advanced in 1849 to account for cholera being so fatal in certain localities in the city of Aurora, and presented to the profession in the spring of 1853. Dr. Snow, of London, in 1854, presented his theory that cholera poison emanated from the evacuations, and that it must be swallowed to produce its specific effect. Pettenkofer advanced his theory several years later.

Again, in that report, speaking of the manner in which the epidemic is spread over the country by persons unwell with choleraic diarrhea, on page 168 will be found the following views: "Was it not cholera producing this diarrhea; and if it was, was not the system laboring under a mild attack of cholera? And when we see persons, as I have before mentioned, unwell with diarrhea, appear to propagate cholera, is it not a rational inference that they would also propagate the disease in the form of diarrhea, when scarcely any cases commence without this derangement of the bowels? Why consider this diarrhea a premonitory symptom, or the forming stage of cholera, as is generally done by our authors, when we see it prevail so extensively as an epidemic, (intimately associated with the disease,) often fatal in a few hours, and appearing, as it spreads over the country, to remove the susceptibility of whole communities to the influence of cholera? When this diarrhea causes death, are we to consider that it was not the disease (cholera) that produced death, but merely the premonitory symptoms of the disease? We have yellow fever without black vomit, and certainly it is reasonable that we may have cholera in a mild form. We consider, then, that cholera may be spreading through a neighborhood, in the form of a mild diarrhea, and public attention scarcely directed to it, when some persons, or a family, more susceptible to its influence, are attacked and several deaths suddenly occur. The inference generally in such cases is that the disease arose spontaneously. If these views are correct, that cholera may be propagated while in the form of a diarrhea, we see how rapidly this pestilence may extend over the whole country, even if only one out of ten contract the disease: which, in some sections of the country, under ordinary circumstances, appears to be about the proportion which is susceptible to its influence; although in towns, or where the infection can accumulate, the proportion is much greater. Again, an individual having a mild attack of this infectious diarrhea goes into a neighborhood where the inhabitants are predisposed to the disease; the person himself is scarcely aware that he is unwell; shortly

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afterward there is a sudden outbreak of cholera, and if it does not occur at the house where this individual is staying, how difficult it would be to trace the manner in which the disease was introduced."

It must be borne in mind that in 1853, when these views were presented to this Society, it was not admitted in our medical works that the diarrhea that accompanies cholera could propagate the disease. Dr. Copland, the ablest advocate of the diffusion of cholera by infection, on page 318, sec. 64, Copland's Medical Dictionary, says, speaking of the relation between diarrhea and dysentery, "Yet, although thus very closely allied in nature and form, they are very often distinct as respects, first, the seat of the disease; second, the symptoms resulting from each. Diarrhea is generally sporadic and never infectious." This statement I believe to be incorrect, for I think there is an abundance of evidence to show that the disease may be introduced into a neighborhood from the evacuations (of a person unwell with a choleraic diarrhea) being deposited where they may communicate the disease; or it is highly probable that the disease may be communicated from the clothing of an individual, as he passes through the community, being slightly soiled by this painless or choleraic diarrhea.

Drawn from that report, and from additional observations, I presented to the Medical and Surgical Reporter, a summary of conclusions, which presents in a few words the views advocated. These conclusions are as follows:

First. That cholera is unlike any other disease, consequently it is governed by laws peculiar to itself.

Second. That its cause is an organic poison, which is reproduced within the human system, and is diffused by human intercourse.

Third. That for the development of cholera on this continent, there must first be cholera poison; next, a community susceptible to its influence.

Fourth. That the susceptibility to cholera resides in the organic nervous system, upon which the poison first makes its impression; producing enervation and altered action of the capillaries, giving rise, in many instances, to profuse exhalations and morbid secretions from the mucous membranes, from which probably the infection arises.

Fifth. That this susceptibility is not alike in all persons, but exists to the greatest extent in a community that has never been under the influence of cholera.

Sixth. That temperature or season of the year, humidity, modes of living, and other local and physical causes, increase or diminish the susceptibility of a community to the disease.

Seventh. That cholera, like other diseases, may assume different grades of violence, and may also prevail under different forms.

Eighth. That it may prevail under the form of a diarrhea. That where predisposition, season of the year, or other causes, favor the development of dysentery, that cholera assumes this form of the disease. That it may pre-

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vail as a mild cholera morbus. And the malignant form is the name that we would give to those cases where there is failure of the circulation, blueness of the skin, cramps, etc.

Ninth. That when cholera poison was introduced upon our continent, in 1832 in the North, and in 1848 in the South, it was the cause of these four forms of disease spreading over the country—in many places intimately associated, and in others appearing only as an epidemic diarrhea, which we consider not merely a "symptom" of cholera, or the "premonitory stage," but *cholera itself in a mild form.* Why call this diarrhea that accompanies cholera the premonitory stage, when a large majority of the cases proceed no further? And at what period of the attack are we to consider true cholera as commencing?

Tenth. That the disease may be spread by each of these forms, and by the diarrheal form even if only one person out of ten is susceptible to its influence. Consequently no other disease known can be as easily diffused over the country as cholera, and in no other disease is it so difficult to trace the manner of its diffusion.

Eleventh. That the malignant form of cholera is favored by a variety of causes, among which are predisposition, tendency to diarrhea, modes of living, mental depression, probably fear, but more particularly than from any other cause, by the accumulation of infection, which overpowers vital resistance before the system has acquired the power to tolerate the disease; as we see similarly, in certain malarial districts, a simple disease assuming the fatal and malignant form of congestive fever.

Twelfth. That the principal cause of the accumulation of infection is from the evacuations, which, from their watery character, are generally emptied upon the ground; and from which the infection is capable of spreading through the atmosphere, and giving a local malignancy to the disease.

Thirteenth. That this accumulation of infection takes place when cholera is introduced under favorable circumstances into large crowds of human beings, or poorly ventilated places, or into cities; or on low, damp situations during stillness of the atmosphere, and particularly where the subsoil is of a clay or limestone formation; while upon a dry and sandy soil, which rapidly absorbs moisture, the infection seldom accumulates.

Fourteenth. That season of the year or temperature has a marked influence upon the progress of cholera; and that during the winter it may prevail in our cities as a mild form of diarrhea, to re-assume its malignant form as summer and other conditions favor its development.

Fifteenth. That the system soon tolerates the poison; and that attacks of either of the forms of cholera, and frequent exposure to the infection, removes, in a great measure, the susceptibility to the disease; which is probably acquired again in from fifteen to twenty years. This power in the system to

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tolerate the poison is the reason the disease leaves the country, and seldom attacks a city or town severely the second time.

Sixteenth. That when cholera poison is once introduced upon our continent, the best prophylactic in our northern climate (in connection with proper sanitary regulations) against the malignant form of cholera in the summer, is frequent exposure, under favorable circumstances, to the infection during winter; by which the susceptibility to the disease may be, in a great measure, removed.

I am well aware that cholera prevailed in Russia, Scotland and parts of England during the winter; but I think that these facts, when properly examined, have but little weight against the correctness of these conclusions, each one of which can be sustained by what appear to me convincing arguments.

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THE COLLAPSED STAGE OF CHOLERA.

BY W. S. HAYMOND, M. D., MONTICELLO, IND.

The rapid extension of epidemic cholera over a large portion of the world during the last two years, has necessarily called to it the special and absorbing attention of the medical profession. And the probability that it will continue epidemically for some time, in the regions lately visited by it, should serve to increase the interest already excited, and lead to more careful observation and investigation. We have made important advances, during the last two years, in the pathology of the disease, but we have not solved the mystery of its origin, nor demonstrated its cause. This can and will be done. Notwithstanding much diversity of opinion still prevails with regard to the nature of cholera, and its pathology and treatment, yet important changes of opinion have taken place as to doctrines that were heretofore scarcely questioned; and new theories, based upon careful observation, and the result of much investigation, have been promulgated, and are now rapidly superceding the old.

At the commencement of the present epidemic it was unpopular to affirm that cholera is contagious. Though this belief is not yet universal, it is gaining ground rapidly, and now claims among its advocates many of the most distinguished minds of the profession. Much more that was taught heretofore concerning cholera has been rejected owing to recent investigations.

The pathology of the collapsed stage of cholera can scarcely be considered apart from its general pathology. The questions must necessarily arise: what is the nature of cholera? what is the peculiar *materies morbi* that awakens it to existence, and causes it to be propagated over vast regions in a short time. Upon these points there rests, unfortunately, much doubt, and we are yet without the certainty afforded by demonstration. Holding the doctrine that contagious and epidemic diseases are specific in their character, and generated from specific causes, I cannot doubt that cholera is produced by some poison, *sui generis*, which enters the system from without.

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According to the enlightened views of modern pathologists contagious exanthematous diseases, as well as diphtheria, typhoid fever and erysipelas, etc., are due to the specific effects of certain blood poisons. Though no chemical analysis or microscopical scrutiny has detected these subtile elements of pestilence, yet their existence cannot be denied, or we must admit that these diseases can exist without a cause. These reasons apply with equal force to cholera. Failure to detect the poison cannot be admitted as evidence of its non-existence. We have so far failed, with the highest power of the microscope, to detect the germs of the fungoids, or certain species of the cryptogamia, yet we know they exist, and are propagated over extensive regions through the medium of the atmosphere. We can, at the present time, judge only of the nature of the cause from its effects. We have strong reasons for believing that the poison of cholera is more potential than any known organic or inorganic matter. The rapid propagation of the virus over vast regions of country; the simultaneous appearance of the disease at points widely separated; and the terrible effects produced by the absorption of an almost infinitesimal quantity of the poison, are facts that admit of no solution except upon the hypothesis that this peculiar morbid agent is indued with the power of reproduction. When the poison enters the system a sensible but variable period of incubation is observed. This may sometimes be very short, but, according to Pettenkofer, may be as long as three weeks. The poison remains in the system until development takes place, when it passes off in multiplied forms, ready for transmission. We are led to conclude, then, from the potential nature of the choleraic poison, and its inherent capacity for reproduction, that it is indued with vital properties; or, in other words, that it is a "living organized germ."

The germ, when introduced into the system, undergoes rapid prolification, and thus effects sundry alterations in the blood, and serious derangements of all the functions of the body. Cholera, then, may be regarded as a zymotic affection, contagious according to the peculiar laws of germ transmission. Among the usual effects produced by the choleraic poison may be mentioned the arrest of all the secretions, except that of the intestines, which is increased. In the stage of collapse the most striking symptons are blueness and almost icy coldness of the skin, pinched features, sunken eye-balls, short and hurried breathing, and clammy perspiration.

It is admitted by close observers that the usual post mortem appearances are denudation of the epithelial covering of the intestines, which are generally pale, and without evidence of inflammation; lungs generally empty of blood and remarkably pale, while the other organs of the body are engorged with dark venous blood; almost complete emptiness of the bronchial arteries and veins; engorgement of the pulmonary arteries; emptiness of all the other arterial channels, and general fullness of the veins; and

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emptiness of the left cavities of the heart, which transmit arterial blood, while the right cavities, from which issue the vessels for the transmission of venous blood, are full.

This relation of the post mortem appearances is in accordance with the declarations of those distinguished observers, Drs. George Johnson and A. E. Sansom, to whose valuable works on this subject I have made free reference. Dr. McPherson states, "on the whole the arterial system is empty, while the venous system is very full of blood."

The most popular notion held heretofore concerning the nature of cholera was that the disease was only exaggerated diarrhea, and collapse the necessary result of excessive drainage of fluid from the blood. This theory I hold to be incorrect. If cholera is due to the action of a specific blood poison—which can scarcely be doubted—we should not overlook the fundamental lesions produced by the poison, and attribute them to secondary effects. It might be asserted with almost equal propriety that collapse is due to the suspended functions of certain secretory organs.

That the cause of cholera may act upon the mucous covering of the intestines to a limited extent, is highly probable, but we cannot attribute the whole phenomena of cholera to the local effects of any poison. Such a theory is negatived by the suddenness and severity of the symptoms in a large proportion of cases; by the excess of the alimentary discharges, and absence of abdominal tenderness and inflammation.

Primary intestinal irritation is certainly not essential in all cases to the production of diarrhea. Fetid gases, from decomposing animal or vegetable matter, entering the blood through the lungs, are capable of exciting diarrhea; which may also be induced by causes that act primarily upon the nervous system, such as violent mental disturbance.

The theory that collapse is caused by loss of fluid from the blood is inconsistent with other well established facts.

First. It is wholly independent of the quantity of the watery discharges; occurring as often when the evacuations are small as when they are most abundant.

Second. Recovery from collapse is more frequent when the watery evacuations continue, than when they cease, or become very scanty. According to Dr. George Johnson, recovery from this stage is enhanced by encouraging free discharges from the bowels with mild cathartics.

Third. Recovery from collapse is often too sudden, and is followed too rapidly by reaction, to be attributed to the loss of fluid.

Fourth. If collapse were solely due to the loss of fluids, we would expect to find the patient suffering from great prostration, with faintness and indisposition to rise and move about. But the reverse is generally true; patients in the most hopeless collapse are often able to rise and walk about, and "perform acts requiring considerable physical exertion."

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Fifth. Collapse sometimes occurs suddenly, without antecedent diarrhea, and before the peculiar rice-water evacuations have appeared.

Hence the theory that collapse is the result of the loss of fluid, fails to account for all the facts, and should be rejected.

Before the phenomena of collapse can be manifested, there must be admission of the choleraic germ into the blood, (which may enter through the lungs by inhalation, or the intestinal canal by absorption,) and subsequent contamination of that fluid, by germ prolification, or zymotic action. The blood being contaminated, acts as a nerve poison, perhaps influencing more particularly the nervous centres and the great sympathetic nerve.

In combating the heretofore popular notion that collapse is altogether due to the loss of the watery constituents of the blood, we may derive some assistance from the analogies afforded by other diseases. The collapsed stage of malignant, or pernicious, intermittent fever is strikingly similar in many respects to the corresponding condition in cholera. In both diseases the phenomena of collapse result from the action of blood poisons; and in both, the nervous system and the functions of organic life are directly molested. The shrunken features, the cold and discolored skin, and clammy perspiration belong to each; the frequent and often indistinct pulse is common; so of other symptoms, as insatiable thirst, vomiting, purging, difficult and hurried respiration, etc.

These symptoms of the two disorders, though differing greatly in severity, and generated by different poisons, point sufficiently to the fact, that the pathological condition denominated collapse, is generally dependent upon lesion of the nervous system, or some part of it. In pernicious intermittent fever, collapse frequently occurs when no morbid action can be traced to the intestinal canal; it also occurs often when the bowels are constipated.

Sansom expresses his belief that "all the phenomena of collapse are due to central irritation of a part of the nervous system; and that purging and vomiting may be due to peripheral irritation of the mucous lining of the alimentary canal, to central irritation of the nervous system, or to both of these causes conjoined." And he very truthfully remarks that "a theory to account satisfactorily for the phenomena of cholera must explain the cause of cholera as well as the cause of collapse," as these do not bear to each other "the relation of cause to effect."

To Dr. George Johnson we are indebted, more than to any one else, for calling into question and refuting the old popular idea that cholera was only an excessive diarrhea, and collapse but a sunken condition of the system resulting from the loss of the watery constituents of the blood. Experience had no doubt taught him, as it must have done others, that treatment based upon this time-honored theory, which necessarily comprehended the extravagant use of opium, astringents, and alcoholic stimulants, was less successful than any method ever tried; whereas, if the pathology had been true, it

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would have been the best treatment. According to his confirmed opinions, the diarrhea and rice-water evacuations are but the efforts of nature to eliminate the morbid material from the system, while collapse is caused by spasm of the small branches of the pulmonary artery—an effect, also, of the irritating properties of the poison.

This latter opinion needs corroboration. There has been no substantial reason offered why spasm should be more likely to affect the branches of the pulmonary artery than other parts of the arterial system.

The objection has been raised against this part of his theory, that it attributes to nature an inconsistency, by assuming that the diarrhea is an effort of nature to rid the system of the poison in one direction, while elimination in another direction (through the lungs) is arrested by a sudden spasm of the only vessels through which the morbid material can be expelled; thus causing it to be retained in the right side of the heart and the vessels communicating therewith. Another objection is the difficulty of conceiving that the poison should be absorbed into the blood, carried through the entire circuit of the venous and arterial systems, and be propelled toward the lungs, "and only evince its power of inducing contraction of the coats of the vessels at the end where the pulmonary arteries terminate in their capillary plexus."

Notwithstanding these objections, his views on cholera have awakened a spirit of deep inquiry, and have done more to revolutionize opinion on this disease than the writings of any other man.

I think a dual action is manifested by this poison, which will readily account for much that has heretofore been the occasion of no little perplexity. On this point I shall again refer to Sansom, with whose views my own closely correspond. He says that "cholera is the result of a poison which manifests its effects in two different ways, according as it is in immediate relation with the alimentary canal, or is absorbed into the circulating blood. As a primary irritant it causes the immediate phenomena of vomiting and purging; as an absorbed poison it compromises the integrity of the great sympathetic nerve, ultimately becomes an irritant thereof, and causes the combined phenomena of true choleraic diarrhea and collapse."

In attributing to the morbid germ of cholera a local irritant effect upon the intestinal mucous membrane, and a constitutional effect as an absorbed poison, I assume nothing beyond what is well known with regard to the action of various other poisons. Many of these are known to be harmless when taken into the stomach, but destructive of life when introduced directly into the blood. There are, again, other poisons which act as irritants upon the alimentary canal, and when absorbed, cause infection of the blood; in either way they are capable of causing vomiting and purging. This theory of a double action of the *materies morbi* will easily account for the phenomena of cholera in its different stages. The incipient diarrhea, which yields often to simple remedies, or disappears without further manifestation of cholera, is merely

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the result of the local irritation of the germ upon the mucous membrane of intestinal canal. Cholera in such cases is arrested because absorption of the poison has not taken place. But the more serious phenomena of the disease, or that assemblage of symptoms which constitutes cholera proper, are wholly due to the absorption of the poison.

Sansom argues, with great plausibility, that collapse and true choleraic diarrhea are due to special implication of the great sympathetic nerve; and that the nature of this disturbance is irritation, but not paralysis; for Brown-Sequard has shown in his Physiology of the Nervous System, that paralysis is characterized by effects totally opposite to those belonging to collapse. I have no doubt this nerve exerts a very important influence in the production of collapse.

The effects attributed to irritation of the great sympathetic nerve, as tabulated by Sansom, viz: enfeeblement of respiration, loss of voice, escape of the watery constituents of the blood, coldness of the surface, arrest of secretion, etc., are identical with the symptoms of collapse. As the poison of cholera is carried by the blood to every part of the nervous system, we should not too hastily infer that any part escapes with impunity. The circle of the great sympathetic being connected with the cerebro-spinal system, thus making the whole nervous system an integer, it appears to me objectionable to attribute an independent action to any part or division. That the great sympathetic is more immediately instrumental in the production of the phenomena of collapse than any other portion of the nervous system, may be readily inferred from the anatomical arrangement of its series of ganglia and nerves of connection, which supply the various organs not under the control of the will. It presides over the functions of secretion, digestion, and circulation; and, according to Brown-Sequard, also, "rythmical actions, movements of the intestines, and in a great degree the functions of respiration; and is a vaso-motor nerve." The sympathetic being a nerve of supply to the arterial system, we may attribute to it a direct agency in effecting contraction of the coats of arteries. These vessels being always found empty in fatal cases of cholera, we may reasonably conclude that the blood is forced out of them by the contraction of their muscular tissues. When contraction commences in the arteries, we may suppose it will continue until the blood is propelled into the capillaries and forced through them into the veins. By this action the entire column of arterial blood is transferred to the venous channels; and this arrest of arterial circulation is undoubtedly one of the most important links in the chain of causes that culminate in collapse. The excessive strain that takes place in the capillaries, on the broad surface of the intestinal canal, by the mechanical force of contraction and propulsion, will account for the excessive liquid evacuations of cholera; and, also, far more satisfactorily than any theory has heretofore done, for the separation and purging away of the columnar epithelium of the intestines.

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These views of choleraic collapse and cholera are submitted as embodying some of the most recent notions of the pathology of this disease, and as an expression of my own belief.

The treatment of cholera has been more discrepant and empirical than that of any other disease. Almost every plan of treatment has been tried, generally in an irrational manner, corresponding, in some measure, with the fanciful and speculative notions entertained with regard to its pathology.

It is evident that our treatment must ever be unsuccessful and unsatisfactory until it is based upon pathology. If cholera is the result of a blood poison, and the evidence is almost conclusive, rational treatment would endeavor to control this morbid condition. If external disinfectants are capable of neutralizing the infectious germs of cholera out of the body, why should they not prove more effective when administered internally? Remedies of an antiseptic nature are clearly indicated. Among this class the sulphites are undoubtedly the most effective. From a scruple to a drachm of the sulphite of soda, dissolved in water rendered slightly aromatic, should be given every two or three hours. This treatment should be commenced as soon as possible, and continued until there is manifest abatement of the symptoms. In place of the sulphite, we may prescribe, as recommended by Sansom, carbolic acid, two drops; chloroform, three drops; acacia mixture, one ounce—every two or three hours.

We should rather encourage elimination through the bowels, than attempt too soon to arrest the intestinal discharges with opium or astringents. Is it ever necessary to prescribe remedies to restrain the diarrhea? It is not proper to resort to them early, or while there is reason to believe elimination is incomplete; afterwards, if the discharges are excessive, we may very properly attempt to repress them.

Having laid down the outlines of the general treatment of cholera, it will more readily appear what course should be adopted in collapse. This may be divided into two classes—the first, embracing those cases in which the symptoms belonging to the early stages of the disease are wanting; the second, those cases preceded by diarrhea and the usual rice-water discharges. In the former we may presume that the arterial channels are not yet empty, and that the duration of the attack has been too short for the total impairment of absorption, and also that time enough may be granted for the elimination of the poison from the system. With this hope, we should prescribe as freely as the stomach will bear, the remedies recommended above. In the great majority of cases of the second class, absorption and assimilation are so completely suspended that it is questionable whether food or remedies taken into the stomach are ever beneficial.

We should make persevering efforts to establish reaction. The application of ice to the body is only calculated to hasten the death of the patient. We should endeavor to preserve the animal heat. Ice and cold drinks

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should be used as sparingly as possible. It is questionable whether ice ever allays thirst in collapse, and if used too freely may greatly increase it, and in reducing the heat interfere with reaction. Heat should be imparted to the body by means of bottles or jugs of hot water, heated bricks, etc., aided at brief intervals with friction of the surface. Counter-irritation with mustard drafts, ammonia, carbolic acid, etc., should be sedulously applied over the greater part of the abdomen, and warm water injected freely into the rectum. In administering enemas the bag and syphon apparatus is much to be preferred over the ordinary method. Besides imparting heat to the system the enemas will serve to dilute the blood, and may be rendered of still greater service by adding disinfectants, and nutriment in the form of beef tea or animal broths. Alcoholic and other heating stimulants which would excite the heart to increased action and cause the small quantity of arterial blood that still remains in its cavities to be propelled more rapidly through the almost empty channels, should not be prescribed. Camphor, however, seems to meet with some favor, even among those who are opposed to the general use of stimulants. Sansom says it cannot "cure cholera, nor can it relieve collapse, but it seems to be a stimulant whose use is followed by rapid warmth, and whose tendency is to dilate rather than contract the capillaries."

Experience has proven that the use of opium in collapse is pernicious. It is contra-indicated by reason of the engorged condition of the venous system, which would be increased by its administration.

As the contracted state of the arteries is one of the most serious features of collapse, any measures which would tend to their relaxation must be clearly indicated. The transfusion of blood, warm water and medicated fluids into the veins has been attended with some success, and merits further trial.

Antispasmodics, administered through the stomach, can rarely prove beneficial, for reasons that have been stated; but the inhalation of chloroform is at least worthy of a fair trial.

Venesection is sanctioned by many of those who have had the most extensive opportunities for observation. It has often afforded sudden relief under the most unfavorable circumstances; but it is a remedy that should not be resorted to until all other means recommended have failed. It offers a feasable method of inducing motion of the contents of the vessels, and as collapse is not simply a condition of debility, the loss of a few ounces of blood, if it is not too thick to flow, can be productive of no harm, offers a chance for recovery, and we should not fail to invoke its aid.

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CEREBRO-SPINAL MENINGITIS.

BY JOHN MOFFETT, M. D., RUSHVILLE, IND.

Within the last few years, there has been prevailing in this country a disease, the phenomena of which are somewhat peculiar. In almost all localities, where it has made its appearance, its ravages were alarming. The first cases were usually fatal; some being stricken down in a few hours, whilst others lingered days and weeks. Such a state of things created in the mind of the profession an unusual interest in its pathology and treatment. The affection is different from any disease to which the attention of living physicians has been directed. Medical literature is meagre upon the subject, and very unsatisfactory. The young practitioner was constrained to rely upon what was already established as proper in the management of analagous complaints.

The first evidences of the presence of this malady in this locality occurred in January, 1863. Children, mostly under one year old, were the subjects of its attacks. For a day or two the child would be fretful, particularly on being moved. This usually continued two or three days, with a gradually increasing indisposition, until some one of the more marked symptoms appeared. At this period, the physician was consulted. In three or four of the first cases observed, the prominent features were the backward and lateral curvature of the spine, accompanied with difficulty of inspiration, as though the action of the diaphragm and intercostal muscles was interfered with. Most of these cases were free from febrile symptoms. The pulse was increased in frequency, and weak, but not indicating dangerous debility. In most of these cases, the bowels were natural; in some, the stools were frequent, with thin, greenish-colored evacuations. The urine was usually highly tinged with lateritious sediment.

Succeeding these cases, in March and April following, others insidious and of more gravity occurred. Sometimes the seizure was abrupt and sudden, requiring but a few hours to develope the most marked case; whilst at other times it would be days before the diagnosis could be made satisfactorily, especially by one who had not seen much of the disease.

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To illustrate the difficulty of a correct diagnosis on the first, or even the second visit, take the case of a little girl in the family of a physician. On Friday she complained slightly of pain in the left hip. On Saturday, the pain had increased so as to demand a critical examination, which was made, but no definite conclusion was arrived at. The father supposed it might be local rheumatism, and accordingly applied a camphorated liniment. The father had to leave home, and on Monday evening, the writer was called. The little patient was complaining of severe pain in the left hip. There were heat of skin, thirst, soluble alvine dejections, tongue unchanged in appearance, pallid countenance, anxious expression of face, dorsal decubitus; the lower extremities were drawn up, and head drawn back; pulse 120, small and rather feeble. She was given opium for the night, and warm hop applications were applied to the hip, hoping light would be thrown upon the case by morning. Nine o'clock the next day, on visiting the patient, I found the symptoms ameliorated; pain entirely gone, fever assuaged, pulse under 100, skin soft and of natural elasticity, tongue moist, and anxiety of expression gone. Still I was in doubt about the result. Quinine, opium and calomel were administered so as to meet a supposed periodicity in the paroxysms. At two o'clock on the same day, a severe rigor came on, lasting sixty minutes, when fever began to rise, culminating in a convulsion. The latter were frequently repeated, until a permanently unconscious state supervened. Great suffering characterized the progress of the malady. Then I began to suspect the true character of the attack, although the eruption which has given a name to the complaint, did not occur during the case, which ended fatally.

A child may appear to be in perfect health, and in four hours have a raging fever, such as will induce the most alarming convulsions. The heart's action reminds one of the peculiar thrill of the organ in vivisections. The heat of skin is most intense, and occasionally there is paralysis of the upper eye-lid, and loss of power in one-half of the body, with wild delirium, or coma. The little patient, in some instances, attempts to imitate every sound it hears. In others the attack is equally sudden, but the constitutional symptoms more mild, with an immediate explosion upon the skin, of the eruption which is said to be pathognomonic, but is by no means constant.

The most constant phenomenon in the cases I have observed is the inordinate action of the heart. This does not accord with the statement of cases reported in the journals, where the pulse is said to be but little changed, and in a few instances below the normal standard. I think that probably the condition of the heart and arteries has not received the attention its importance demands in this affection.

The movement of the heart is peculiar. The arteries partake of its nervous expansion and contraction. When there is a very high grade of fever, often the artery dilates to the fullest extent, but there is very little resistance to pressure.

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One other characteristic of this disorder needs special notice: the early disposition to opisthotonos, which constitutes so painful and unpleasant a feature. The degree of backward curvature indicates the progress of the disease.

The eruption, when it does obtain, is peculiar. The number of cases which I have seen has not been great, but sufficient to familiarize the eye with the distinctive features. The period of eruption varies from the first to the eighth day; perhaps it occurs more often on the second and third days, than any other date. It comes in purple or blue spots, varying from the size of a flea-bite to large irregular blotches, with very little, if any, elevation of the cuticle. Frequently these spots are thickly studded over the entire skin, but in the greater number of those I have seen, the eruption was not so general. The more usual seats were the extremities, and the lower parts of the trunk. In case of recovery, where the eruption is a symptom, it disappears, without leaving any mark.

Patients who are old enough, and rational, complain of extreme soreness on attempting to move, as though all the voluntary muscles were as rigid as the erectors of the spine.

Dr. W. W. Gerhard says: "Between typhus and spotted fever there are some points of resemblance; both these diseases attack subjects of any age, are nearly equally free from pathological lesions, and are each attended by a peculiar eruption. Still there are well marked diagnostic characters which show a broad distinction between these diseases. These are as follows: in spotted fever the disease is very rapid in its course, with delirium rarely of an active kind; an eruption wholly different from that of typhus; less heat of skin, which is never of the burning temperature of typhus, and none of the peculiar odor of that disease. Typhus offers a true exanthematic eruption; but I have already described that of spotted fever to be rather a scorbutic hemorrhage than a real exanthema. Besides, the duration of typhus fever is nearly as regular and as long as typhoid; whereas in spotted fever the course of the disease is irregular, and generally much shorter. The difference in the mortality of the two disorders, and the unequivocally contagious character of typhus fever, are also additional points of diagnosis."

Dr. Lamb says: "The general aspect or physiognomy of the patient is so remarkable that when once seen, it cannot be mistaken. It would be very difficult to describe the anxiety and intense suffering exhibited in the countenance of the patient. The jactitation is incessant, and the suffering without intermission."

The most prominent features of the disorder now being considered are referable to the nervous system, and more especially to the spinal cord. This part of the nervous tissues in an especial manner presides over reflex action, and serves to conduct sensitive and motive impressions to and from

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the brain. The anatomical arrangement of the cord is much the same as that of the encephalon.

In Dr. Lamb's case, sixty-three hours after death, the brain and its membranes were intensely congested throughout; the brain softened, excepting the pons varolii. An effusion of yellow serum or lymph was noticed beneath the arachnoid, between the convolutions of the brain and in the spinal canal. In Dr. Frick's case, a large ecchymosis was found under the pericranium near the saggital suture. The vessels of the dura mater were filled with dark fluid blood, which could readily be pushed aside by the handle of the scalpel. The substance of the brain, and of the medulla oblongata, was natural in appearance and consistence. There was no effusion into the ventricles, or inflammatory exudation. In Dr. Jewell's case, the vessels of the scalp were more than naturally filled with blood. The vessels of the dura mater were remarkably congested with fluid blood very dark in color. A yellow effusion existed in the subarachnoid space and in the spinal canal, serous in character. The substance of the brain was firm, with very little, if any, congestion. In the lateral ventricles there was a somewhat abundant reddish serum, and on cutting into the substance anywhere the gap would soon fill up with serum exuding from the cut surfaces.

Dr. Liddell found, in his case, while removing the brain, that at least six ounces of serum, colored with blood, flowed away. The glandulæ pacchioni were unusually large, numerous and adherent, for a person twenty-eight years of age. A moderate quantity of limpid serum was beneath the visceral arachnoid membrane, especially about the vertex. The arachnoid had lost its transparency and become more or less opaque, especially in that locality. A moderate amount of serum was in the ventricles; the choroid plexus in the fourth ventricle was thickened, pale red in color, and presented a striking resemblance to a lamina of pale, flabby, granulations; but the choroid plexus in the other ventricles presented no abnormity. The cerebellum, pons varolii, and medulla oblongata were carefully examined. They were congested throughout, but presented no other abnormal appearance. On sawing through the posterior vertebral arches, and removing them so as to open the spinal canal almost its whole length, it was seen that the theca vertebralis was distended, or at least well filled with serum, notwithstanding the large amount of cerebro-spinal fluid which had escaped while examining the brain. On laying the theca open, it was found that the spinal arachnoid membrane had lost its smooth, shining appearance, and was ossified throughout its whole extent. The subarachnoidian blood vessels were every where intensely congested. The cerebro-spinal fluid was noticed to contain some thin, white flocculi in the lumbar region, but otherwise it was limpid. The structure of the spinal cord did not present any abnormity.

All who have recorded any thing in relation to the blood, state that it is disposed to fluidity; the coagula usually found in the sides of the heart, are

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soft and easily broken up, and the blood lodged in the smaller vessels is in a fluid condition. The kidneys are ordinarily highly congested. In addition to these it is not uncommon to find petechial spots, or vibices, upon the liver, stomach, mesentery, and other organs within the abdomen and thorax. The blood, no doubt, is the first to receive the morbific impression induced by the active cause of this specific disease. When the circulating fluid is so modified, the next step in the chain of morbid phenomena is hyperemia of the meninges of the spinal cord, extending rapidly to the envelopes of the brain. If there is hyperemia of the vessels of the meninges of the cord, the circulation of its substance will be incited to increased activity, and there will be developed at an early period the fixed condition of the muscles of the spine, and the attending pain; and by continuity of surface and similarity of texture and function, the brain and coverings will alike be involved. Thus headache is present early in the disorder. Here lies the danger—the tendency to stupor and thence to profound coma. How shall the invariableness of these two symptoms be accounted for in fatal cases?

"Where coma results from pressure within the cranium, this must act alike upon the cerebrum and sensorium."—*Carpenter.*

If this is correct, something is known of the pathological condition of the cerebrum and sensorial centres. There obtains from the inception a determination to the cerebro-spinal axis. There is inflammation of those nerve centres and their envelopes engrafted upon some prior change in the blood, and the treatment must be conducted accordingly. For further proof as to the nature of this disease, a few statements of Brown-Sequard, as to the symptoms and effects of chronic meningitis of the spinal cord, will not be out of place here:

"First. Pain usually of a rheumatic character, and more or less diffused along the course of the spine. Generally there is no great increase of this pain when pressure is applied on the spinous process, as in case of myelitis; while on the contrary, every movement of the spine forward, backward or laterally, increases the pain, sometimes to a very great degree.

"Second. The nerves originating from the part of the spinal cord where the meninges are inflamed, are the seat of acute pain, which is very much increased by mechanical causes. The sciatic nerve, for instance, when meningitis exists in the lower part of the spine, seems to be the seat of very great pain, which is much increased when the lower limbs are moved. When inflammation is confined to the part of the membranes at the place where the nerves of the upper limbs originate, the movements of those limbs cause great pain.

"Third. There are frequent, and in some cases, constant spasms in the muscles of the back. These spasms which render the spine almost immovable, are produced or increased when an attempt is made to move the trunk. When they exist in the cervical region they bend the head backwards, just as in tetanus.

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"Fourth. Anæsthesia is very rare in spinal meningitis. Sometimes there is a real hyperæsthesia, or at least a morbid sensibility and an increased susceptibility to reflex movements."

This narration of symptoms will serve to throw light upon the disease in question. They corroborate the opinions I have expressed in this paper of this perplexing disorder.

A case in point was seen with Dr. John J. Inlow, of a female, unmarried, sixteen years of age, of good general health. After a long walk on a very cold day, she complained of general soreness and stiffness, which lasted from Friday until Monday afternoon, when she had slight febrile action, pulse somewhat accelerated, bowels rather costive. The most marked symptoms were the muscular soreness, aggravated when movements were made, and pain along the upper portion of the spine, without much tenderness manifested under pressure. Friday night following, the patient fell suddenly unconscious, and no exhibition of pain could be induced by the movement of the extremities. Before my arrival in consultation, a blister had been drawn over the dorsal spine, which measurably restored consciousness and the muscular soreness and pain, the least movement being attended with great suffering. We concluded that the meningeal coverings of the spinal cord were congested, with danger of subacute inflammation. Active counter-irritation, and mercurials and anodynes internally, were used, and recovery followed.

That this disease is a *cerebro-spinal meningitis*, engrafted on some previous change in the blood, is certainly, in the present state of pathological inquiry, the most rational conclusion. The condition of the blood probably consists in a lowered vitality, and hence the slow progress towards the development of inflammation in the parts affected. Those who die early in the disease show no signs of inflammatory products. The frequent eruptions internally and externally, resemble those effusions generally conceded to depend on a depraved state of the blood, such as the petechiæ of typhus, purpura and scurvy.

The measures best calculated to control the disease are of the first importance. Two indications should be kept steadily before the mind of the practitioner, namely: first, the restoration of the blood to its normal condition; second, to protect the brain and spinal cord from the results of inflammation.

I am aware that I differ from many eminent observers in maintaining the ultimate tendency of the local disease toward inflammation, and I do so with much diffidence. If the blood is depraved in its nutrient qualities, and there is a disposition of the solids to rapid debility, the abstraction of the vital fluid, either locally or generally, is contra-indicated.

Dr. Gerhard's plan of treatment consists in applying external heat in the cold stage, and afterwards placing sinapisms to the extremities. Blisters to

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the nape of the neck may sometimes be applied, but are of doubtful utility. The head should be sponged with cold water, and occasionally cold may be more permanently applied. The most important remedies are stimulants and quinine, the latter given in doses of one or even two grains every hour.

Dr. Liddell says: "These patients died from effusion of serum upon some part of the cerebro-spinal axis. Now any plan of treatment, to have been successful, must either have prevented the effusion of the serum, or caused its absorption prior to the occurrence of fatal compression of the nervous centres."

Dr. Hartshorn says: "It is to the constitutional affection, and not to the mere local disorder, that our remedies must be directed. It is worse than useless to treat these accidental inflammations as ordinary idiopathic phlegmasiæ, and I cannot but believe that our main reliance in the treatment of all these morbid phenomena must be placed in the free exhibition of quinine, brandy and turpentine.

When attempting to combat this disease by means of remedial agents, two circumstances should be kept in view, in fulfillment of the indications already named. The first is to introduce into the blood, as rapidly as may be, those articles which experience has long since shown to be efficacious in maintaining its vitality. The second consists in the local application of such agents as will secure permanent revulsion. Quinine, iron, brandy, whisky, and chlorate of potash, are the chief means internally. Opium is only useful in cases where the febrile excitement does not run high in the onset, and in in the later stages of all cases, where the irritation and suffering is great. In the language of Liddell, "opium furnishes the means of preventing effusions of serum in the nerve centres." Of the good effects of opium in suitable stages of the disease, I do not doubt. Chlorate of potash and alcohol furnish oxygen to the blood. Iron is needed by the red corpuscles. Observation has shown these articles to be serviceable in the treatment.

In regard to the local treatment, counter-irritation perseveringly applied has proved, in my efforts, to be one of the most powerful measures for the good of the patient. Blistering must be kept up for weeks together, to secure the advantages to be derived from revulsion. In case of an actual effusion of blood, creating an artificial irritation upon a more healthful surface is not likely to enhance the destructive tendency of the effusion, or precipitate it where it does not exist. If the effusion is not prevented, or removed after it has taken place, death will ensue. What measures can be put in motion so likely to prevent or remove exudations in the spinal apparatus? I am persuaded, from observation, that each successive renewal of a blister has been followed by a marked relaxation of the backward curvature of the spine, and diminution of the accompanying pain. Without such a proceeding, I have no assurance of the

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restoration of my patient. I do not believe one application is of any particular benefit. Counter-irritation must be continued until convalescence is complete. I have kept a blistered surface open for four weeks in succession, and, as I believe, for the salvation of the patient. My experienee attests the usefulness of quinine, liberally given, to abate the heart's action and heat of skin. Its mode of action is not easily comprehended. M. Briquet is of opinion, from his own experiments, and from the evidence of other observers, that sulphate of quinine is not a stimulant, but a cooling and calming remedy. This judgment is inadmissible, if applied to small doses of the medicine; but appears to be correct as regards large doses. Stillè says: "It is a tonic stimulant when given in doses of medium size, as it is a sedative when administered in large doses."

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FEMALE DOCTORS.

BY DOUGAN CLARK, M. D., RICHMOND, IND.

Woman is emphatically an aggressive being. However much we may deceive ourselves by styling her the "weaker vessel," and trying to confine her within a "sphere," she is still a power in the world. The man promises to "love, cherish, and protect" the woman, but occasionally finds that she is fully able to "protect" herself, as well as to extend a friendly guardian care over him; the woman promises to "love, honor, and obey" the man, which not unfrequently signifies that—

> "She'll suffer him still to hold the reins,
> But she'll show him which way to go."

It is not probable that there will be a sufficient number of "strong-minded" women at any early period to "stop the population," as they sometimes threaten to do when a real or supposed right is denied them; that is to say, it is probable (as I believe Fanny Fern remarked some years ago,) that there will always be weak-minded women enough to keep up the population, and, I may add, to furnish some obstetric practice to those persons—male or female—who are qualified to perform it. But women, as well as men, can carry a point in very many ways besides "stopping the population;" and, so far from being a reproach, I consider it an honor to the sex that they give indisputable evidence of possessing a strong force of will. The fact has been expressed briefly and pithily by a certain rhymer—

> "If she will she will, you may depend on't;
> And if she won't she won't, and that's the end on't."

Let no one suppose that I am here to cast any reflection upon the female or even to jest at her expense. On the contrary, I have the most sincere esteem and admiration for the sex; and I rejoice to believe that there are thousands of women in the nineteenth century who will compare favorably with the men, not only in intelligence and virtue, but also as regards fixedness of purpose and decision of character. The elevation of the female has

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been very gradual, and is to be attributed, in a great measure, to the influence of Christianity. It is true that, in ages far back in the past, individual women attained to distinguished positions on account of remarkable gifts—as Miriam and Deborah for the spirit of prophesy possessed by them; Aspasia, the counsellor of Pericles, for her wisdom; and Lucretia, the celebrated Roman matron, for her virtue. According to Tacitus, the ancient Germans—a totally uncivilized race—regarded their women with a kind of superstitious veneration, supposing there was something sacred and prophetic belonging to them, and even worshipped certain ones as goddesses. Such instances, however, are exceptional, and nearly all nations, civilized or savage, which have not received the light of Christianity, have regarded the woman rather as the slave than the companion and equal of the other sex. Slowly and steadily, however, she has been winning her way to a loftier position. Female education is no longer confined to branches which are ornamental rather than useful. The solid and abstruse sciences are not only studied, but learned by girls as well as boys. Women, as well as men, are found in the library of the scholar and the studio of the artist. Small and delicate hands guide the pen of the philosopher and historian, and "soft eyes" are seen "rolling" in the "fine frenzy" of the poet. Many occupations for earning an honest livelihood, and many avenues to distinction, which were formerly monopolized by men, are now open to the gentler sex, and we trust the time is approaching when the absurd and unjust distinction of giving to females lower wages for equal services will be abolished.

For a number of years past there have been indications quite unmistakable that woman was preparing herself to invade the medical profession. Applications for admission to this or that medical college have been made by females at different times, and in nearly all instances refused. I suspect there has been a disposition to refuse them superciliously and arrogantly, not to say contemptuously. I shall be glad to know that I am mistaken in this supposition. I only infer it from the fact that several years ago I heard Professor Meigs, in an introductory lecture, ridiculing unmercifully the idea of females turning doctors. I should be slow to believe, however, that that distinguished Professor would address a respectable lady, who should wish to purchase a ticket for his lectures, in any other than terms of politeness and courtesy—even though he might refuse her request. He would doubtless reserve his ridicule for the class room. Quite recently two ladies who made application to be received into the medical department of Harvard University, were denied by the trustees in a very polite note, on the ground that there was no provision for the education of females in any department of that University.

The first female graduate in Medicine on this continent, I believe, was Dr. Elizabeth Blackwell, who received a diploma from the Medical College of Geneva, and is now enregistered as a British practitioner. One English

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female also, Dr. Garret, has a medical diploma from the Society of Apothecaries. The door by which she entered has, however, since been closed; and so the recognized medical institutions of Great Britain and our own conntry are united in excluding female aspirants for medical honors from participation in the benefit of their instructions. In the meanwhile, however, several females in both countries have, by some means or other, attained to the status of respectable medical practitioners. Some of them are acknowledged by the profession to be possessed of "attainments and abilities of the highest order." We may instance Lucy Abbott, of New York, and Ann Preston, of Philadelphia, either of whom, I suspect, in a rigid examination on the science and art of Medicine, before a board of censors, would eclipse a large proportion of our medical graduates; and there are probably some dozens of ladies, located in different parts of the Union who are equally well qualified. Now I think there can be no question as to the gross injustice of refusing recognition to persons like these, because they have not entered the profession in the regular way by receiving the endorsement of a medical faculty, when, at the very same time, the privilege of earning such endorsement has been positively denied them. They have gained their knowledge, not by the aid of medical colleges, but in spite of them; and if we rigidly close the door of the Temple of Medicine against female applicants, it is certainly hard to stigmatise them as "thieves and robbers" if, in their anxiety to reach its beautiful shrine, they "climb up some other way."

The question of extending the usual courtesies to worthy female practitioners is forcing itself upon the profession, and it cannot much longer be evaded. A very interesting discussion of the whole subject recently took place in the Philadelphia County Medical Society, and was ordered to be published in the medical journals of the city. It seems that in 1860 the State Medical Society of Pennsylvania had adopted a resolution prohibiting its members from "consulting, or holding professional intercouse" with professors or graduates of female medical colleges as "at present constituted," on the ground that "some of the professors are irregular practitioners, and the colleges ineligible to membership in the American Medical Association." At the meeting of the same society in 1866 another resolution was introduced, which allows members to "consult with regularly educated female physicians who observe the 'Code of Ethics.'" This resolution was referred to the several county medical societies, with instructions to report thereon at the next meeting of the State Society. A committee had been appointed, accordingly, in the Philadelphia County Medical Society, to report upon the status of female physicians. Said report was read by Dr. S. Francis Condie, chairman of the committee. It consists of a preamble and a resolution. The former assumes the ground that females are competent, if properly educated, to practice medicine, or to do anything else; that, however, they should be discouraged from becoming physicians, because God did not intend them for

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that profession—but, on the contrary, to be "good nurses, good citizens, good christians, and ornaments to society;" that while everything which is open to man is open also to woman, if she is properly educated for the purpose—and if she will insist upon being a physician, and is properly educated, we will receive her and consult with her—yet female medical colleges, as now organized, are very poor affairs. If we are to have female physicians they should be educated at the same schools where the males are professionally trained, either in the same classes, or in separate classes at different hours. The resolution appended reaffirms the original action of the State Society, and denies professional recognition to graduates of female medical colleges "as at present constituted."

In the discussion that ensued, those opposed to the resolution asked "How are females to be educated in our male colleges? Have they not applied year after year, come to our doors and begged to be received, and been rejected? In self-defense they had to organize their own college." And it might have been added, that owing to the antipathy of the profession against the whole scheme, it was difficult, if not impossible, to get first-class men to fill the professorships. It was urged on the other side that at least one of the professors in the Female Medical College at Philadelphia had been playing the quack most thoroughly, and an advertisement was read setting forth the infallible virtues of his "Compound Asiatic Balsam." (The editor of the Medical and Surgical Reporter, however, in a recent issue, assures us that he is no longer a professor in the college, and that from that institution the stain of quackery and irregularity is entirely removed.) One member remarked that "There are female graduates who are a disgrace to the profession, and who ought to be punished by law;" to which Dr. Ann Preston, in an able and dignified letter to the Reporter, replies that "For the sake of humanity, we would that we could truly add that the graduates who disgrace the profession are found only among women." At the close of the discussion it was resolved unanimously that "The members of this Society cannot offer any encouragement to women becoming practitioners of medicine, nor can they consent to meet in consultation such practitioners." The subject had occupied the attention of the Society for two successive meetings, those of March 13 and March 20.

On April 13 the Montgomery County Society took up the same subject, and its members came to a decision precisely opposite to that of their brethren in Philadelphia. They resolved that "We invite woman, with her quick perception, her varied talents, her deep sympathies, her patience, her fortitude, and devotion to every good work, to join us in our strife with disease and death." They instructed their delegates to the State Society "to use all honorable exertions to place respectable female graduates and the professors of female medical colleges on an equality with male graduates and the professors of male medical colleges." They also resolved that "The

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State Medical Society should insist that all medical colleges in the State should admit to their halls every properly educated person of good moral character, without regard to sex, ethnical peculiarities, or nationality."

These facts render it quite evident that the subject is engaging the attention of the profession, and we may just as well examine its merits now as at any other time. The question will have to be met; it cannot be postponed very long by any summary process of ruling out, such as was recently employed in the American Medical Association. The subject is claiming the attention of British practitioners as well as American. A correspondent of the British Medical Journal revives the following lines from Spencer's Faery Queen, as apropos to the subject. I quote from the Medical and Surgical Reporter:

> "So prospered the sweet lass; her strength alone
> Thrust deftly back the dislocated bone;
> Then culling curious herbs, of virtue tried,
> While her white smock the needful band supplied;
> With many a coil, the limb she swathed around,
> And nature's strength returned, nor knew the former wound."

Whoever this "sweet lass" may have been, she was evidently no bungling surgeon.

I repeat once more that the question is before us. The fight has begun, and it is about time to assume a position on one side or the other. If we believe that there are very grave objections to the introduction of females into the practice of medicine, we should promptly say so. If we believe that the welfare of society, the welfare of the profession, and the welfare of themselves, alike imperiously demand that they should be excluded, let us use our utmost endeavors to exclude them. But I am strongly inclined to believe that our efforts will be in vain. I suspect that in this contest the weak are to conquer the strong—the few are to conquer the many. I suspect that the broad ground of merit and qualification is to furnish, in the future, the only criterion for admission to, or exclusion from, the fellowship of medicine. And now, having tried to bring the matter fairly before my professional brethren, I should be sorry for any one to suppose that I am personally in favor of women engaging in the study or practice of medicine. My prejudices and feelings are all opposed to it, and yet I must confess that increased examination of the subject has tended to diminish opposition in my own mind. I believe the wear and tear of a physician's life would, as a rule, inflict too severe a strain on the delicate physical and mental organization of the female; while it must at the same time prove, to some extent, incompatible with the proper performance of those domestic duties which render woman the charm of the home-circle and the proper helpmeet of man. I would not advise any female in whom I had a special interest, to become a physician; just as I would not advise her to become a farmer, nor a blacksmith, nor a carpenter, nor even a lawyer; but if she determines to qualify

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herself for any of these occupations, I insist upon it that she shall have a fair opportunity and equal privilege. We may employ advice and influence with entire freedom; still there will be women who will act altogether independently of our advice, and who will not be controlled by our influence. There are to-day females of as good moral character as you or I—with as much sense as you or I—with as much scientific knowledge as you or I—practicing medicine with as much skill and and as much success as you or I—willing to be bound by the same code of ethics as you and I—and their number is constantly increasing. Shall such as these be excluded from all professional recognition, and courtesy, and counsel, and assistance, simply and solely because they are women? If it is a fixed fact that there are to be female doctors, is it not to the interest of all parties that they shall be as efficient as possible?

I fully appreciate the difficulty of giving instruction, in some of the important branches of medical science, to females in mixed classes; but I have no doubt that this difficulty can be obviated by the good sense and experience of those who shall have the courage to undertake it; and I should think it much better that they be educated at our leading institutions, which are stantly drawing into them the best talent of the profession, than in separate schools of their own, which, for a while at least, must necessarily be inferior. Time and a fair trial, however, will determine this point. Let us, then, be willing to act justly, even though we do not feel like acting generously. Let us admit females to the profession on precisely the same ground as males, and it may be that we shall find that after all we have been wrong in our prejudices. It may be that we shall discover that woman has peculiar qualifications for some departments of the healing art, and that from her we shall gain an important accession of strength in our life-time combat with sickness, and suffering, and death.

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TRACHEOTOMY

IN CYNANCHE TRACHEALIS, DIPHTHERIA AND LARYNGITIS.

BY R. E. HAUGHTON, M. D., RICHMOND, IND.

The operation of tracheotomy was recommended by Antyllus, about A. D. 340, and was previously practiced by the Asclepiades, in diseases of the throat threatening suffocation. It was, for a long time subsequently, advocated upon theoretical grounds, but its value was not put to the test. Even after it had been tried for a long period, but one instance of successful result was recorded, "performed in 1782, by Mr. Andre, of London, on a little girl five years old." In 1825, M. Bretonneau, of Tours, saved a little girl in the last stage of croup by this operation; and it has been performed many times since, by different operators, with encouraging success. The most of the successful cases occurred in France; in England, the result in croup has been so unfavorable, that it has been regarded as unjustifiable. In estimating the results of this operation in France, it must be recollected that in many instances it was performed on patients whose disease was probably amenable to other treatment; and in some cases the trachea was opened without the adoption of any previous treatment, in an early stage of the disease. While we should not infer that all the patients on whom tracheotomy was performed would have died if it had not been resorted to, these cases prove it to be, in itself, not very dangerous.

Whatever may be the result of an early operation, it is only practicable in a hospital; and we must estimate it with reference to private practice; in which we are very liable to commit the error of waiting too long; so that if it is done at all, it is when the condition of the patient is the most unfavorable for success, the nervous system being profoundly injured, and often irreparably so.

This operation does not cure, but, by sustaining the respiration, may enable the patient to wear out the disease. When the disease is confined to the larynx and trachea, and the obstruction to respiration is such as to demand

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immediate relief, it is necessary to consider the propriety of an operation, and, if required, to perform it. When there is difficult respiration, from disease of an inflammatory character, and which is liable to extend by continuity of structure to the bronchiæ and air-cells; or there is any organic disease of the lungs, as tubercular softening with abscess; or in that form of croup which sometimes occurs in convalescence from measles or scarlet fever, when there is great lividity of surface from imperfect aeration of blood, an operation should not be attempted. The blood-poison, in the latter cases, has so impaired the functions, that, as I have witnessed, an operation only hastens a fatal result.

It might be supposed the same would be true of diphtheria, also a zymotic disease, but sufficient success has been obtained by the operation in this malady, to justify the opinion that sometimes life may be saved by its timely performance.

There is nothing in the nature or history of either croup or diphtheria to prevent us from resorting to tracheotomy. In laryngitis, when remedies fail and suffocation is imminent, it should be performed. Cases of erysipelatous laryngitis are on record, in which an operation proved successful; and in such cases, the probability of failure is greater than in uncomplicated ones.

Chloroform, as recommended by Dr. Foch, should always be resorted to, as it renders the operation far more easy of performance, and he has never, even in extreme dyspnœa, found any ill effects from its employment. At first the dyspnœa is increased by the inhalation, but as soon as the anæsthetic influence is established, the breathing becomes calmer than before.

The time for performing this operation, in this country, at least, is deferred until death by suffocation is impending; and I believe this is the cause of so many failures, and the reason why medical men resort to it with hesitation. To postpone it until the patient is almost dead, is cruel, and discreditable to surgery. When remedies can effect no good, and there is an increase of all the urgent symptoms, it should be performed. The earlier an operation can be performed, when we have so decided, before the disease has exhausted the vital forces, the more likely will it be to succeed. If there be no complications beyond the trachea, the earlier it is done, the less is the danger of the further extension of the disease.

Failure, in this operation, in many cases, arises from the impossibility of sufficient air finding admission through the opening made in the trachea. The opening should be large, and the tube should be as large as the trachea will permit. This is the opinion of M. Trosseau, and he gives this as a reason for the relapse which occurs so frequently after the most encouraging temporary improvement. Air is admitted sufficient to afford temporary relief, but not enough for perfect aeration; and when failure again manifests itself, it is by hurried breathing, the reappearance of the livid hue of the

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surface, and feebleness of pulse, which betoken imperfect depuration of the blood. If a healthy person, who is in the habit, during sleep, of breathing with the mouth open, awaking with dry tongue, tries to breathe through the nose alone, he is oppressed with a sensation of want of air that makes him open his mouth to respire. Again, take a tube of the size of the tracheotomy tubes, and breathe for a time entirely through it, and soon oppression occurs, which is only relieved by a return to ordinary respiration. This, Trousseau says, happens when an opening of inadequate size is made into the trachea: air enters; the spasmodic efforts of the muscles of the glottis are somewhat relieved; but after a few hours, or days, the symptoms recur and the patient dies. Acting upon these views, he always makes a much larger opening, and introduces a much larger canula than the majority of surgeons.

The operation should be performed slowly and deliberately, the tissues divided layer by layer, and the trachea well exposed before opening it.

After its performance, the most important things to be done are, to secure quietness and a proper temperature for the patient, and to prevent any obstruction to the tube or canula which has been inserted. In many cases, the obstruction to the tube has been very difficult to overcome, because the canula used has been too small. Constant care is required to prevent inspissated mucus from closing the end of the tube. A large double silver canula should be used; or what would be better, an elastic spring, passing around the neck, separating widely the edges of a large opening; we thereby get rid of the irritation of the tube. Thus would the accumulation of mucus or pus escape more readily. Moist air at a temperature of 65° or 70° should be breathed during the treatment. Warmth, nourishment, tonics and stimulants are indicated to support the strength and favor nutrition. We should not be in a hurry to remove the tube, or close the opening, as time is required to restore the natural breathing.

I have not collated statistics with reference to laryngitis, but only in croup and diphtheria, which involve the same tissues, produce the same dangers, and require the same promptness in operating for their relief.

The following statistics, which sufficiently explain themselves, show that the results of the operation in diphtheria differ but slightly from those in croup; and that a little more that one-fourth of all the cases of both diseases, operated upon in Europe and America, have been saved by tracheotomy.

Tracheotomy in Croup, at Magdeburgh Hospital, Germany, by Dr. Foch.

Cases operated upon	24
Recovered	10
Deaths	14
Per cent. of recoveries	41.66

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Table of Cases of Croup in which Tracheotomy was Performed.

OPERATOR.	OPERATIONS.	RECOVERED.	DIED.	AUTHORITY.
Trosseau	153	41	112	
Bretonneau	18	4	14	
Baudelocque	15	.....	15	
Guersent	9	.....	9	
Pancoast	7	3	4	
Amussat	6	.....	6	
Gerdy	6	4	2	
Petit	6	3	3	
Blandin	5	.....	5	
Velpeau	6	.....	6	
Roux	4	.....	4	
Maslicurat	2	1	1	
Page	1	.....	1	
Smith	1	.....	1	Phil. Med. Ex., 1852.
Thompson	1	.....	1	N. E. Med. Jour., 1816, vol. v., p. 318.
E. Atlee	1	.....	1	W. J. Med. & Phys. Sci., vol. iv., p. 23, 1831.
Townsend	1	.....	1	
Van Buren	1	.....	1	
Buck, Jr.	1	1	.....	New York Med. Jonr., 1851.
Johnson	1	.....	1	Richmond, Va., Stethoscope, 1851.
R. H. McIlvaine	1	.....	1	
J. G. Pitney	1	1	.....	N. J. Med. Rep., 1852.
Dan. Ayers	.....	.....	.....	N. Y. Jour. Med., 1852.
Bigelow	1	.....	1	Am. Jour. Med. Sci., 1853.
G. A. Hinkler	1	.....	.....	Western Lancet, 1856.
R. E. Haughton	1	.....	1	Report to Medical Society, 1865.
Totals	250	58	191	Per cent. recovered.....23.20.

Statistics of Tracheotomy in Croup at the Hospital des Enfants in Paris, in 1859, 1860 *and* 1861.

YEAR.	OPERATIONS.	RECOVERED.	DIED.	PER CENT. RECOVERED.	MEAN SUCCESS IN THREE YEARS.
1859	164	40	124	24.39	
1860	130	23	107	19.23	
1861 (10 mo.)	82	25	57	30.48	
Totals	376	88	288		24.70 per cent.

Cases of Croup Treated in Hospital Saint Eugenie, from March 15, 1854, *by Bouchut.*

	BOYS.	GIRLS.	TOTAL.	PER CENT.
Number of cases treated	305	229	534	
Number of cases operated on	211	163	374	
Number of recoveries after operation	36	28	64	17.25.
Number of deaths after operation	175	135	310	
Cases not operated on	94	66	160	
Recoveries	33	25	58	1 in 2.44.
Deaths	61	41	102	1 in 1.58.

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Table of Cases of Tracheotomy in Diphtheria.

OPERATOR.	OPERATIONS.	RECOVERED.	DIED.	PROPORTION OF RECOVERIES.	AUTHORITY.
Trosseau	250	60	190	1 in 4 1-6.	
Bretonneau	20	6	14	1 in 3 2-3.	
Bouchut	160	45	115	1 in 3 1-2.	
Velpeau	10	2	8	1 in 5.	
Spence	54	19	35		Am. Med. Jour., vol. xvi., p. 526, 1848.
Buchanan	15	5	10	1 in 3.	
R. E. Haughton	5	1	4	1 in 5.	
Totals	509	137	372	1 in 3 7-9.	

Total number of cases operated on, in this paper.....1,533
Recovered 357
Per cent. of recoveries..... 23.28

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EPIDEMIC DYSENTERY

IN ALLEN COUNTY IN 1845, 1854, 1856, 1864.

H. P. AYRES, M. D., FORT WAYNE, IND.

The attention of the State Medical Society has frequently been called to the subject of dysentery; and no subject demands a closer investigation. Many interesting features have been presented, yet many points remain unsettled.

The treatment of dysentery must ever be interesting to the observing practitioner, especially in our miasmatic country, from the fact that our climate is changeable in character.

No disease so largely contributed to fill our military hospitals and disable our soldiery during the recent war, as did dysentery, and its kindred disease, diarrhea. Many cases were undoubtedly not reported, and yet the numbers returned were more than one-fourth of all the cases of sickness. The total number of cases reported during the first year of the war was 215,214. During the second year, 510,461. The greatest number of attacks and deaths occurred in what is called the "central region," in which our own State was embraced. The maximum month, during the year ending July, 1862, was August; and during the year ending June, 1863, was July. The report of the Surgeon General shows, that while the Atlantic region presents a higher degree than the central, the latter keeps at a more uniform grade, thus showing a greater prevalence of the disease.

In the general hospitals of Ohio, Indiana, Illinois, Iowa, Missouri, Kansas, and Kentucky, including those at Cincinnati, Louisville, and St. Louis, the mortality was one death to every nine (8.53) cases; in those of Tennessee, Northern Mississippi, and Arkansas, including, among others, the hospitals of Nashville, Memphis, and Helena, one in five, (5.4); in the hospitals of Louisiana, including New Orleans and Baton Rouge, one in four. In the hospitals of Keokuk, Iowa, the mortality was one in nine cases (9.1); in St. Louis,

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one in five (5.2); in Cairo, one in four (3.98); in Memphis, one in five (5.4). In the report, Cairo shows the greatest mortality, which gradually diminishes until we reach Keokuk, Iowa; Indiana being a medium between these extremes.

It may appear almost presumptuous to report on a subject which has received so much attention from our State Society and medical associations, and especially from eminent writers in this and other countries. Studious investigation has characterized the interest of the profession in this subject. Years of patient labor have been insufficient to satisfy inquiries respecting its pathology and treatment. Ever varying in its phases, it has exhausted the resources of the materia medica. At one time it is mild and manageable; at another time it sweeps a neighborhood or town. Age, sex, season and locality are often put at defiance. Of dysentery it may be said, wherever man is found there will some of its forms appear.

The special pathology of dysentery, with its anatomical lesions, has long been settled; the colon and rectum being the points where the greatest derangement of the economy occurs; yet it may extend from the mouth to the anus, involving all the glandular and peritoneal textures of the abdomen.

From what causes the disease originates, how the poison is introduced into the system, the changes it produces in the organs of respiration and nutrition, how it developes itself in the colon and rectum, and why it assumes its various phases in different years, are questions in which we, as physicians of Indiana, are deeply interested. Why, during one year, is it mild, making its attacks upon all in the same manner, however differently situated; and, during another year, does it assume a chronic, typhoid, or inflammatory type? If heat, cold, food, or exposure, are productive of the disease, why do they not always produce such results? Why have we dysentery of the same type in the extremes of temperature?

The strong fall before the march of dysentery as rapidly as the weak, and the weak recuperate as rapidly from its attacks as the strong. We can come to no other conclusion than that there must be some agent or agents acting, whose phenomena and results are similar wherever they are exercised. When the poison producing it finds its way into the system, the results are as surely developed as are the effects of mineral or vegetable poisons.

Every physician knows that affections of the various organs of the body produce phenomena peculiar to themselves, which are always present. The characteristics of dysentery are the same in all countries and in all latitudes, and there must be similar causes to produce such uniformity, but changed by the hygienic state of the body, or affected by local influences.

Dysentery is but the manifestation of another disease. The primitive diseased part of the economy is the blood, which may have been changed from its normal condition by poisons. However the disease originates, by the law of

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election it invariably manifests itself upon the colon and rectum. It may be perpetuated by the poisonous secretions or excretions of the alimentary canal, frequently disappointing the expectation of the physician by a recurrence of the attack.

Neither the existence of digestible nor indigestible food in the alimentary canal are necessarily productive of dysentery. Every practitioner of experience has observed children, during the prevalence of dysentery, using unripe fruit and other substances equally pernicious with the utmost freedom, and yet entirely escape the disease. We also, in lienteric states of the bowels, observe food passing off without change, yet without any appearance of dysentery.

Parents often consult the profession respecting the best means of preventing dysentery during its prevalence, and yet their families may be, perhaps, the greatest sufferers; while often those who are indifferent entirely escape. We do not assume that care is no safeguard to health; but where the producing poison enters the body the results are positive and certain, regardless of the most watchful care.

In the treatment of dysentery there have been the most antagonistical views in reference to curative powers respecting almost every article in the materia medica. Remedies which have proved highly successful in one year, have proved inefficient in the succeeding one. The present treatment is largely of an experimental character. The identity in dysentery, wherever found, we think, should lead to a more successful and uniform mode of treatment; but, until investigation more fully reveals its pathology, we must content ourselves with our present remedies.

Among the remedies which have been most extensively used mercury and opium stand prominent in all parts of the world, but with very diverse results from their use.

Dysentery always appears in one of two forms: acute or subacute. The chronic and typhoid states, which we frequently see, may result in either form of attack. In the acute form we may have a febrile state of the whole body from the onset; or we may have a cool skin, with a rapid pulse, and great thirst; or we may have a burning skin and no very marked acceleration of the pulse. We may have tenderness of the bowels or intense pain in the head, or both; but always, in this form, restlessness and loss of appetite.

In the subacute variety we may have all the phenomena of dysentery upon the colon and rectum, with very little pain, either in the abdomen or head—the patient sometimes appearing entirely free from any unpleasant sensation, unless from tenesmus; while the appetite may remain unimpaired, and no increase of thirst be present. A morbid state of the stomach and an engorged liver or spleen, only exist as complications.

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During the year 1845 dysentery prevailed in Northern Indiana as an epidemic, and assumed a decidedly inflammatory character. The stomach, the whole alimentary canal, and the liver, were involved in the trouble. There was great tenderness of the bowels, and in many cases severe tympanitis, with an excruciating pain in the anterior part of the brain. For one or two days there was a slight disturbance of the bowels. At the close of the second day there was a free serous discharge, succeeded in a few hours by a sanguineo-mucous, then by a sanguineo-purulent one. Great nervous sensibility, restlessness, rapid emaciation, cadaverous features, and sometimes aberration of the intellect, were present. The pulse was full and hard at the commencement of the disease, with an increase in frequency of thirty or forty beats per minute. The tongue was dry, the thirst insatiable, and fluids were frequently ejected from the stomach as soon as swallowed. The whole organism appeared to be involved in a general inflammatory action, which, when unrestrained, soon brought on dissolution. I have noted several cases as they occurred in my own and other physicians' practice, as illustrative of the facts in question.

Case First.—Miss E. S., aged 23, was robust and fleshy, with ruddy cheeks, dark hair, and of a cheerful disposition. She had, for two days, slight diarrhea. On the third day, the discharges rapidly assumed a muco-sanguinolent character, (often clear blood,) with severe and increasing tenesmus. The bowels became painful and tympanitic; the urine was voided frequently, but in small quantities; great pain in the cerebrum, spasmodic in character. During the paroxysm, the pain was excruciating. There was a sufficient quantity of blood taken from the arm to soften the pulse, which, for the time, entirely relieved the pain. A stimulating liniment was freely applied to the bowels and back, followed by soft light flannels, wrung out of a strong decoction of hops, and kept warm. The closet-stool, before being used, had a handful of hops thrown into it, with two or three pints of scalding water, the steam of which was very grateful to the patient. Dover's powder and ipecac were administered every three hours, in sufficient doses to produce slight nausea, and to some extent to mitigate the pain, yet they did not entirely control the action of the bowels. To allay the thirst, the patient was permitted to drink freely of mucilaginous ice water. After the inflammatory stage had disappeared, hyd. cum. creta and acet. plumbi. were added to the former prescription, which effectually controlled the disease. She recovered, with entire loss of her hair as usually occurred that year.

Case Second.—Miss H., aged 19, bilious temperament; stature short, stout; black hair; ruddy skin; cheerful in manner. The dysentery was ushered in with the same phenomena that characterized the preceding case. The pain in the abdomen was severe, with great tenesmus, and a hot, dry skin. The case was treated with calomel and opium; first a large dose, followed by

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successive smaller ones, combined with astringents, to check the action of the bowels. The result was, that the tenderness and tympanitis were increased; the symptoms were aggravated, and the patient died. The cases in which the treatment was first of an antiphlogistic character, with but few exceptions, recovered; while those in which no attention was paid to the inflammation, more frequently died.

Case Third.—Child two years old. Dysentery commenced with slight diarrhea. Second day, sanguineo-mucous stools appeared with tenesmus. Calomel and Dover's powder were given to procure bilious discharges; castor oil to assist their operation. Injections of starch and laudanum were used to relieve the tormina, but were immediately ejected. The calomel not producing healthy operations, was repeated, which only aggravated the symptoms, and the patient died. The whole rectum had the appearance of rings, red, and deeply congested; the abdomen and bowels were largely distended with gas.

Case Fourth.—John E., aged 15; a healthy boy. His attack was a severe one. Cold water injections were assiduously given by his family, and had the effect to check the bloody discharges from the bowels. The fourth day, he became very weak and restless. Becoming alarmed, on the sixth day, medical aid was called. The dysenteric discharges had been checked, but there was a constant regurgitation of bile. The skin was of a leaden hue, the eyes sunken, the nose pinched, and the countenance anxious. Hyd. cum. creta was administered, and in a few hours, he passed healthy fæcal matter, and the regurgitation ceased, but he died. Had proper attention been early paid to the functions of the stomach, liver and intestinal canal, the lad would probably have recovered.

This year, adults were generally attacked, and more women then men. When children were attacked, the cases were very manageable.

In 1854, we were again visited by dysentery, but it was different from that of 1845, the grade of inflammation being subacute, and children generally being attacked. Physicians at first did not meet with success, many of the remedies aggravating the disease. The treatment found best was to evacuate the bowels by some cathartic; then use astringents, which uniformly resulted in convalescence. This treatment in 1845 would have been fatal.

In 1856, we were again visited by dysentery. In that year another mode of treatment was required. Cathartics were necessary, and patients rapidly convalesced under their judicious administration. The disease was confined to children, with an occasional exception. Any derangement of the bowels, teething, or any indisposition, appeared to terminate in dysentery. In this year, the mercurial treatment was decidedly successful; and the majority of cases, after a single dose, would rapidly recover. In cases which were neglected, the tendency was to a typhoid state, more particularly among adults.

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In 1864, dysentery appeared sometime in July, and continued with us for several weeks. In August, it assumed the form of an epidemic, and continued that month. The disease was remarkable for the speedy and perfect prostration of the majority of those attacked. This did not appear to come from the amount or number of the dejections, or from the tenesmus, as much as it did from the manifest failure of the whole organism. In a few hours after the severity of the disease had set in, the patient would become emaciated and cadaverous; yet the mortality was very small. The appetite, in most cases, immediately failed, and continued absent throughout. Thirst was a prominent symptom, the patient desiring the coldest water, or ice. Another characteristic of the disease was the large amount of blood voided, which may account for the sudden and great prostration so common to all. There was a tendency in the disease to assume the obstinacy so characteristic of camp diarrhea, and sometimes I thought it partook something of its nature. I could control the character of the discharges best by the following remedies: ℞.—Syr. aurantii ℥ iij, sulph. acid. dilut. ʒ i. Give a tablespoonful to an adult, a teaspoonful to a child, every four hours. To the above I added the following opiate: ℞.—Pul. ipec. et opii ℈ i, acet. morph. gr. ij. Make six powders, to be given according to circumstances. For an external application: ℞.—Camph. soap liniment ℥ ii, oil of turpentine ℥ i, oil of hemlock ℥ i, chloroform ʒ ij. To be applied every three or four hours to the abdomen and back, sometimes in conjunction with a cataplasm to the bowels. The treatment during convalescence was more stimulating than that usually adopted.

Thus, in our observation, we have found this disease differing every year, and requiring a change in treatment. Here, we think, lies the reason of the great discrepancy in the treatment of this disease: the peculiar type or character has not been sufficiently illustrated, in which calomel, venesection, ipecac, opium, and the various remedies which have been and are reputed as specifics, have been successful.

Dysentery should occupy the same place in our nomenclature as pleuritis, gastritis, or metritis. In each of these the proper treatment has long been settled by the profession, with no better reason than would apply to dysentery. It is not necessary, in all cases of these diseases, to use any one medicine or course of treatment; and an intelligent man carefully investigates the particular case, and applies his remedies, but always keeps in mind its inflammatory character. Place dysentery among our inflammatory diseases, and the treatment will become as uniform as that of pneumonia or pleuritis.

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